# Antiquity

## A Quarterly Review of Archæology

*Edited by*
*O. G. S. Crawford, C.B.E., Litt.D., F.B.A.*

NO. 124 DECEMBER 1957


| | Page |
|---|---|
| Editorial Notes | 185 |
| Evolution and Man. By Ronald Singer | 188 |
| Tools Makyth Man. By Kenneth Oakley | 199 |
| The Evolution of Society. By V. Gordon Childe | 210 |
| Evolution and Diffusion. By Leslie A. White | 214 |
| Worms and Weathering. By R. J. C. Atkinson | 219 |
| Civil Service Archaeology : A Review. By Sir Mortimer Wheeler | 234 |
| Akkadian Tablets in Minoan Dress. By Cyrus H. Gordon | 237 |
| Some Recent Books and Articles | 241 |
| Reviews (*see overleaf*) | 242 |


Published at Ashmore Green, Newbury, Berks., England

*Price : Single Copies 7s 6d—Annual Subscription 30s (or 5 dollars)*

## REVIEWS


Page

*Haithabu, ein Handelsplatz der Wikingerzeit. By* Herbert Jankuhn – – – – – 242
*The Prehistory of East Africa. By* Sonia Cole – – – – – – – – – 244
*Ancient Italy. By* G. M. A. Richter – – – – – – – – – – 245
*Excavations at Nuzi: Vol. VI.* E. R. Lacheman (Editor) – – – – – – 245
*Indo-European Languages and Archaeology. By* H. Hencken – – – – – – 246
*An Outline History of the Nile Valley Sudan. By* Chater Bosayley – – – – – 248
*Das Pfahlbauproblem.* W. V. Guyan (Editor) – – – – – – – – 248
*Greek Theatre Production. By* T. B. L. Webster – – – – – – – – 250
*Eretz-Israel: Vol. III* – – – – – – – – – – – – 251


## EDITORIAL NOTICES


ANTIQUITY is published quarterly on the 1st of March, June, September and December.

The ANNUAL SUBSCRIPTION (including postage) is ONE POUND TEN SHILLINGS Sterling, or FIVE DOLLARS, payable in advance to ANTIQUITY, Ashmore Green, Newbury, Berks., England. SINGLE COPIES may be obtained for *7s 6d* (post paid).

*Everyone wishing to subscribe, or to obtain single copies, is asked to write direct to the Publisher,* H. W. EDWARDS, Ashmore Green, Newbury, Berks., England. Tel. Thatcham 2105.

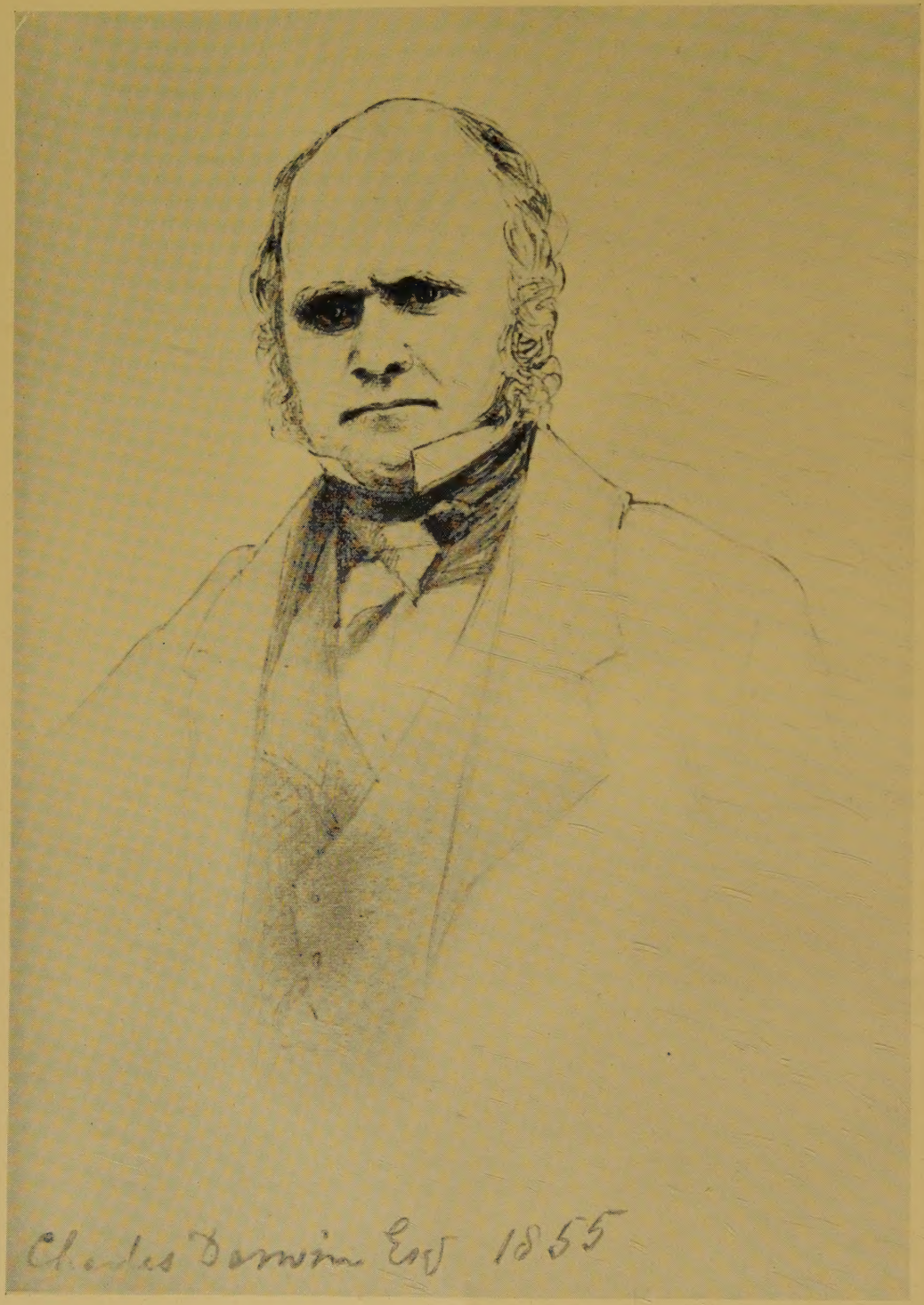
MSS (whether in the form of Articles or Notes) offered for publication should be sent to O. G. S. CRAWFORD, Nursling, Southampton, England, and a stamped and addressed envelope enclosed. All MSS should be typewritten. The Editor is not responsible for the opinions expressed by contributors.

Contributors can help the Editor:

1. By sending a neat and legible typescript.
2. By confining proof-corrections to mistakes. Alterations of the text can only be accepted in the most exceptional cases, e.g., the discovery or publication of new matter.
3. By returning the corrected proof at once to the Editor, by air-mail from overseas.

BOOKS for REVIEW should be addressed to the EDITOR OF ANTIQUITY, Nursling, Southampton, England.

As ARTICLES are paid for, offprints are not supplied except on demand; they may be bought from The Whitefriars Press Ltd., 26 Bloomsbury Way, London, W.C.1, if ordered at least three weeks before date of publication. Offprints of Notes are not supplied.




CHARLES DARWIN IN 1855; FROM AN UNPUBLISHED DRAWING

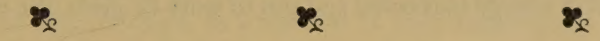
Copyright by ANTIQUITY

# Antiquity

VOL. XXXI No. 124 DECEMBER 1957

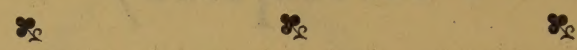
## Editorial Notes

NEXT year the world will be celebrating the centenary of the Origin of Species, first made public by Darwin and Wallace at a meeting of the Linnaean Society in London in 1858. It was at this time that Boucher de Perthes' epoch-making discoveries at Abbeville, derided by his French colleagues, came to the knowledge of Englishmen who accepted them. The next year (1859) John Evans reported on his visit to Abbeville, and his acceptance of the discoveries as genuine and as proving the antiquity of man to be far greater than had hitherto been supposed (see Dr Joan Evans's article in ANTIQUITY XXIII, No. 91, September 1949). The two discoveries were closely related because Darwin's provided the theoretical background that explained *how* the human species could have evolved from some earlier, pre-human, animal. His theory has long been accepted by all those qualified to judge of it; and in the present Evolution number we publish articles dealing with those aspects which concern students of the history of man.

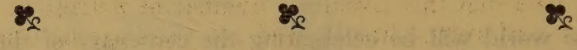


In spite of popular ideas on the subject, archaeologists as such are not qualified to discuss the biological ancestry of man, though they *do* often unearth the evidence by excavation. That is the business of human anatomists, and it is discussed here in an article by Dr Ronald Singer of Cape Town University. Archaeologists are concerned with the evolution of human culture—a word which covers everything from stone axes to ideas and social organization. The essential thing to bear in mind is that the physical evolution of the human body slowed down almost to a standstill with the invention of tools. Up to that point the survival of human (or pre-human) beings depended entirely upon favourable adaptations of their bodies to the environment in which they lived. To take an imaginary example: What would have happened to a toolless mankind overtaken by an Ice Age? Natural selection would have weeded most if not all of them out, certainly all those who could not withstand the cold or migrate to warmer regions; and it would have favoured those with hairy bodies. Eventually, if time allowed, everyone would have been clothed in a thick natural coat of fur. But this did not happen. Tools had been invented and fire discovered; both are (in this sense) tools; and instead of growing fur men used the skins of other animals, thus short-circuiting the need for any physical change. Having protected themselves against the cold by clothing and fire, it may well be that the harder conditions of life on the edges of the ice-sheets, in conditions

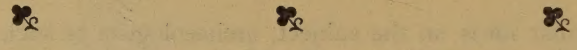
otherwise favourable for the life of hunters, may have acted as a stimulus to invention and so led to more tools and an ever-increasing control of the environment. Dr Pei has had some similar ideas about Sinanthropus and his simian cousin, Gigantopithecus, who lived soft and became extinct; see the current number of *L'Anthropologie* (Vol. 61, pp. 77–83).



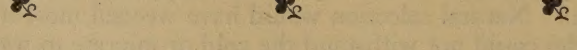
Whatever actually was the sequence of events, it is certain that the invention of tools marked the beginning of human culture. The essential difference between human and animal evolution is that a bodily adaptation (such as growing fur) takes hundreds of generations before it can become really effective, whereas a protective invention (such as clothing) becomes effective at once and can be passed on by teaching to the next generation. You can't teach a child to grow fur, but you can teach him to wear a fur coat.



Thus it comes about that, in all the later stages of human history, the emphasis shifts from physical evolution to the evolution of culture. ' Culture,' said Professor Leslie White ten years ago, ' is a continuous process which grows and develops in accordance with principles of its own. We are thus able to formulate the laws of this development. And the basic law relates to energy. All living organisms require energy. In order to maintain their existence, organisms must be able to control and to utilize energy in one form or another. In the human species, culture is the characteristic means of harnessing energy and of putting it to the service of man's needs. By means of tools and weapons, social organization and knowledge, man is able to harness the forces of nature, and to put them to work for him. It follows, therefore, that as more and more energy is harnessed, the more highly developed does the culture become. When only a small amount of energy is controlled *per capita*, the culture will be low; man will be a savage or a barbarian. If, however, the amount of energy harnessed and put to work be great, the culture will be high.



'We see, therefore, that civilization has developed because ways and means have been found from time to time to increase the amount of energy *per capita* under man's control and at his disposal for culture building. This is the fundamental law of the growth of civilization.' He proceeds to show how the sources of energy used have advanced from, first, those of the human body, then that of domesticated animals, to power production; and that ' the social systems of mankind are closely related to their underlying technological systems—to the ways in which energy is harnessed and put to work.'



Archaeologists and cultural anthropologists, therefore, study tools. One of the first people to do so in England was General Pitt-Rivers, who started by being commissioned to report on the development of the musket and means of improving it. He then applied the results to the implements of ' savages ' (so called then) and of prehistoric men; and he found that the same principles could be observed, namely, that they evolved from simple to complex by the gradual accretion of small improvements. It is now realized that this

process of gradual evolution was often speeded up, and sometimes replaced, by the incorporation of ideas borrowed from another culture—a process called Diffusion, discussed more fully in the present number (pp. 214–18) by Professor Leslie White himself.

Others who contributed towards the building up of this new science were Tylor, who shares with Lewis Morgan the honour of being the founder of anthropology, and Lord Avebury, who, as Sir John Lubbock, did much to popularize it. (We have to thank his daughter, the Honourable Mrs. Grant Duff, for giving us the copyright of the hitherto unpublished sketch of Charles Darwin, used as the Frontispiece of this number.)

It is very appropriate that this Evolution number of ANTIQUITY should contain an article on the action of earthworms, first studied 120 years ago by Charles Darwin. Mr. Atkinson here records the close observations of soils that he has made during his recent excavations, chiefly on Salisbury Plain. Earthworms have in their time moved more soil than all the dirt-archaeologists put together ; and their activities have a most important bearing upon the problems of stratification which confront every excavator each time he digs.

In our next number (1 March, 1958) we shall publish a review of the Editor's book, *The Eye Goddess* (Phoenix House, £2 10*s*.), published last October. We would also repeat, for the information of new readers—of whom we welcome many since our last issue—that their subscriptions for 1958 may be renewed now, on the form inserted in this number. ANTIQUITY's Christmas present to you is an extra four pages—68 altogether—of good reading. Yours to us will, we hope, be a renewal of your subscription for 1958. If there is no form it's because you pay by banker's order, and so have nothing to worry about. Owing to a change in the British law about cheques, receipts are no longer necessary, so we are not sending any in future. You can help us budget for 1958 by ' doing it now '. We have a fine lot of stuff to print, and shall continue to keep you informed of Prof. Cyrus Gordon's exciting researches. Meanwhile a Merry Christmas and a Happy New Year to everyone.

As we go to press the death is announced of Professor Gordon Childe, an article by whom is printed on pages 210–13 of this number. He will be mourned by archaeologists all over the world, not least by the writer of these words, who had known him for over thirty years. Obituary notices appeared in the London *Times* for 21 October and following days.

ANTIQUITY XXXI, 1957

# Evolution and Man

by RONALD SINGER

*Anatomy Department, University of Cape Town*

## INTRODUCTION

THE whole of modern biology has been called ' a commentary on the *Origin of Species* ' (Charles Singer, 1949). In a sense this is true. Following the endeavours to trace the natural histories of the various living organisms, attempts are still in progress to determine the modes, patterns and directive forces of evolution. The end of the 19th and the first quarter of the 20th centuries were characterized by morphological studies in comparative anatomy, the rise of geology and the birth of genetics. The second quarter of this century has witnessed a phenomenal expansion in technical advances leading to critical appraisals of previous concepts and to maturation of new, revolutionary theories based upon seemingly disconnected disciplines—experimental embryology, genetics, physical anthropology, palaeontology and geology. One of the unacclaimed causes of the correlation of knowledge is the post-war mastery of air travel. The spectacular rise of the ' basic ' biological sciences due to emergent industrial and atomic competitive needs in an era of socio-economic enlightenment is another factor giving rise to the pursuit of such knowledge. In a general sense this is the end of a Darwinian ' cycle '; the favourable socio-political situation of the 19th century formed the ' overture ' to the Darwinian theory. Act One saw the development, championing and triumphs of the intellectual interpretations of ' Darwinists '. In Act Two the weaknesses and the vital issues of the application of the theory to various living forms and particularly to *Homo sapiens* were exposed, mainly through the clashes of ' neo-Darwinists ' and ' neo-Lamarckists '. This led to Act Three in which the various sciences (and especially genetics), competing to illustrate and develop alternative theories of evolution, blossomed out, particularly in their search for the mechanisms of the evolutionary processes. In the final scene of this Act the socio-political situation once again formed an important background as the diverse disciplines combine to unify concepts, and, in fact, to *prove* evolution.

The scope and results of a century of thought and research to determine the ' origin of species ' have been amply recorded and analysed in numerous tomes and articles, as a glance at the References on pages 197–8 shows. Only a brief review of the current major concepts and controversies in ' human ' evolution which are still centred around the application of Darwin's views will be dealt with in this paper; the mechanisms of emergence of man from the Primates, the emergent forms, and the status of the Neandertals.

## DARWINISM: BEFORE AND AFTER

The metaphysical dogma of the explanation of the nature and diversity of the forms of life initiated by Aristotle was replaced by the Christian hypothesis of the special creation of static pattern which went unchallenged and unverified for 15 centuries. The 17th century witnessed the onset of thinking in the direction of the historical progression (phylogeny) of living forms. Among the earliest ' pre-Darwinists ' who proclaimed evolution as a reality, though with quite insufficient evidence, may be mentioned Hooke,

Ray, Buffon, Erasmus Darwin and Lamarck (who is second only to Charles Darwin in the history of the evolutionary theory). The optimistic belief of liberalism in the progress of the human race had its true origin in England (Nordenskiöld, 1929), where throughout the entire 19th century prosperity and enlightenment increased, and where humane legislation and democratic social development were demanded and even achieved without violent upheavals. Free competition both in the material and in the spiritual world and no interference with the individual's liberty of action were the watchwords of the age. Evolution, Progress were in fact the slogans. They had been employed in Comte's system in relation to human culture, and through Charles Darwin evolution was elevated to a natural law governing all life. Darwin actually drew the essence of his Theory of Natural Selection from an economist, Thomas R. Malthus, whose chief work *Essay on the Principles of Population* embodied a theory of competition. This explained how the increase of populations in geometric progression never has followed, and does not follow, its natural course, but is restricted, mainly because the supply of maintenance is limited. As a result hunger, war and disease thin out a great number of a community. Darwin stated: ' Being prepared to appreciate the struggle for existence which goes on everywhere, it struck me that favourable variations would tend to be preserved, unfavourable to be destroyed. The result would be the formation of a new species.' The major features of Darwin's theory are:

(1) Natural selection;
(2) Inheritance of the results of use and disuse of organs (Lamarck's principal platform), as a factor secondary to and possibly acting in conjunction with natural selection.
(3) External circumstances which may directly influence the individual's reproductive organs and thus affect the offspring; and
(4) Variations which may arise spontaneously.

Herbert Spencer's application of the word ' Evolution ' has become synonymous (though not correctly) with ' Darwinism '. Similarly the Darwin-Wallace Theory of Natural Selection has unfortunately become linked with Spencer's phrase ' Survival of the Fittest ' in the sense of ' nature red in tooth and claw '. In fact, it is the ' fit ' and not necessarily the fittest or strongest that survive.

The modern concept of natural selection is not quite that of Darwin. He recognized the fact that natural selection operated by differential reproduction, but he did not equate the two. In the modern theory natural selection is differential reproduction, plus the complex interplay in such reproduction of heredity, genetic variation, and all the other factors that affect selection and determine its results (Simpson, 1952). Nevertheless, it has taken 100 years to confirm Darwin's amazing intuition (based on relatively sparse facts), and today natural selection, mutation (' spontaneous variation ' of Darwin) and environmental factors are still the keys to the problem of human evolution.

It is now recognized that the cause of an evolutionary event is the *total* situation preceding it (Zimmerman, 1948), which may embody ' factor complexes ' or ' constellations '. The common denominators of most (probably all) evolutionary histories are gene mutation (i.e. alteration of the gene without loss of ability to reproduce itself, so that when reproduction occurs the altered structure is copied) and recombination, chromosome changes, restriction of population size, natural selection and isolative mechanisms, while time and length of generations and environmental factors are also most important. The record of half a billion years of life have thus been analysed and from this has crystallized the present acknowledged pattern of vertebrate evolution from fishes to mammals (FIG. 1).

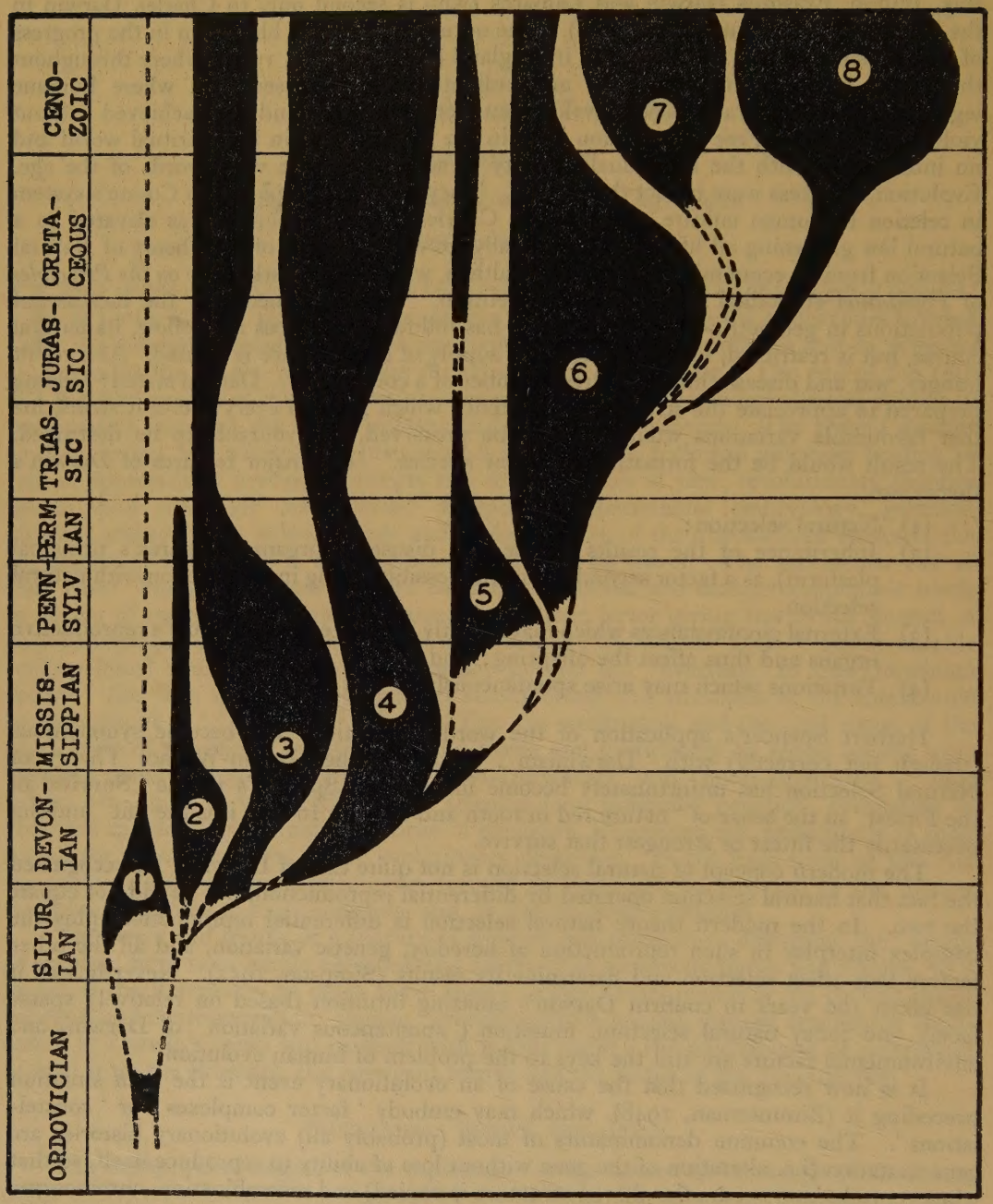



FIG. 1. A FAMILY TREE, SHOWING THE TIMES OF APPEARANCE AND RELATIVE ABUNDANCE AT VARIOUS ERAS OF THE VERTEBRATE CLASSES. (AFTER ROMER, 1954.)

1. Jawless fishes. 2. Armoured fishes. 3. Cartilaginous fishes. 4. Bony fishes. 5. Amphibians. 6. Reptiles. 7. Birds. 8. Mammals.

## Emergence of the Hominids and Hominoids*

In the Paleocene, about 70 million years ago, from the great diversifying upsurge of primitive mammals, whose evolution was favoured by the decreasing numbers and types of Mesozoic reptiles and by favourable ecological factors, there radiated the earliest Primates. These were arboreal shrewlike creatures (insectivores) of such primitive and generalized type that they can hardly be distinguished as Primates. There was also a wide variety of terrestrial and burrowing herbivores, frugivores and insectivores. Thus arose a basic stock specialized in numerous directions. Some soon underwent a progressive evolutionary development (manifested especially in the expansion of the brain, shortening of the snout, and limb modifications to enable efficient grasping) and eventually gave rise to arboreal creatures of the lemur type (Le Gros Clark, 1949) and a great variety of tarsiers. These small Primates (prosimians) spread over a great part of the New World and the Old World, and many formed aberrant lines which ended in extinction. The terminal part of the Eocene and the early Oligocene witnessed a sharp reduction in the diversity of the prosimians because of the competition from other evolving mammals and the progressive lowering of the atmospheric temperature which resulted in unfavourable ecological changes. The tarsiers were superior to the lemurs in that they became better equipped to deal with the progressive amount of environmental stimuli requiring efficient touch, handling and vision apparatuses rather than smell for which the lemurs were better adapted. Natural selection favoured those primate lineages and populations that possessed stereoscopic vision, opposable thumbs and reduced brain area for smell. As a consequence some tarsiers showed a progressive trend in their development which allowed for evolutionary advances that led to the appearance of true monkeys (cercopithecoids of the Old World and ceboids of the New World), while another tarsioid line produced the primitive apes (the pongid line). The earliest known fossil ape (*Parapithecus*) is derived from the Oligocene (about 40 million years ago) in Egypt. Some consider this form to be a representative of the ancestral stock from which all the Hominoidae (including Man) derived. *Proplio-pithecus*, from the same geological horizon, seems to represent a slightly advanced form in the sequence leading to modern anthropoid apes. By the Miocene a variety of ape-like animals were spread over Africa and Asia. In the early part of this period Africa seems to have formed a particularly suitable evolutionary breeding-ground. From Kenya comes *Limnopithecus* rather resembling the gibbon (now confined to South-Eastern Asia), and the larger *Proconsul* which appears to be on the ancestral line of the chimpanzee. Their limb bones indicate that they lacked the brachiating specializations of modern apes. From Middle and Upper Miocene deposits of Africa, India and Europe, the remains of the fossil apes *Dryopithecus* and the gibbon-like *Pliopithecus* have been recovered. In the Lower and Middle Pliocene of India a remarkable variety of apes existed, the most significant being *Dryopithecus*, *Sivapithecus* (also found in Kenya), *Ramapithecus* and *Bramapithecus*. The several dental fragments of the various species of *Dryopithecus* recorded reveal considerable variation, so that some resemble the living orang, some the chimpanzee and others the gorilla, while the cusp pattern of the molars adumbrate those known in fossil ancestors of *Homo sapiens*. It is conceivable that some of the Miocene apes are the actual remote ancestors of Man. However, the view is also held that the Dryopithecine canine was too large and specialized, so that the human line of evolution must have emerged from the ape line before this specialization had developed (compare also with Hürzeler *infra*). But there is a rather large range of variation of the canines in

* ' Hominid ' refers to the human family (Hominidae), while ' Hominoid ' refers to the whole group including Man and the anthropoid apes.

the Miocene and Pliocene ape fossils available at present, so that in some it was less projecting and pointed than it is in some modern apes. In modern *Homo* the canine displays evidence indicating evolutionary derivation from a larger tooth. Le Gros Clark (1950) considers that there is no inherent objection on the basis of the palaeontological evidence to the derivation of the Hominidae from a group of Miocene or Pliocene apes not very different from some of those already known. It is possible to envisage an approximate morphological (not necessarily evolutionary) sequence leading from types such as *Parapithecus* and *Propliopithecus* to *Limnopithecus* and *Pliopithecus*, from primitive anthropoid apes such as *Proconsul* to the various genera of the Dryopithecinae, and from a progressive Dryopithecine type such as *Ramapithecus* to the precursors of the Hominidae. However, Hürzeler (1956) and a few others, on the basis of the *Oreopithecus* remains from the Pontian of Tuscany (equivalent to the French Upper Miocene—Hürzeler, 1956; Lower Pliocene —Straus, 1953), maintain that the basic hominid stock already evolved in pre-Miocene times. Hürzeler states (personal communication, 1956) that *Oreopithecus* displays, at least (see also Hürzeler, 1956) the following hominid affinities: ' high ' mental foramen, a rounded symphyseal region of the mandible, vertical position of the upper canines, anterior position of the malar bones, second premolars as large as or larger than the first premolar, and only one cusp in the latter. It is not relevant to discuss this matter at length in this article, but it should be stated that Hürzeler does generalize extensively without detailed comparisons, e.g., the unicuspid character of the first lower premolar is a character of the primitive mammalian dentition as a whole (Le Gros Clark, 1955). Consequently, Hürzeler believes that Hominid forms already existed on the *Proconsul* horizon when the pongids were already differentiated.

Nevertheless after examination of the available *Oreopithecus* originals, the author hesitates to accept these conclusions based on the scanty ' squashed ' and fragmented material, in which it is not certain whether the cranial and post-cranial fragments are related to each other. The author tends to concur tentatively with Robinson's views (1956) that *Oreopithecus* does not fit easily into any of the known categories: cercopithecoid, pongid and hominid; but its affinities are least close with the hominids and closest with the cercopithecoids. It may be regarded as a late-surviving early cercopithecoid which had not acquired all the characters of this group, because it occurs at a time level subsequent to that at which true cercopithecoids first appear in the known records. It should be pointed out that the Miocene pongids also show a number of cercopithecoid affinities. More detailed studies of the recently discovered Oreopithecine material and exacting comparisons with post-cranial remains of the Miocene apes are awaited. These may assist in clarifying the problem.

Straus (1949) also theorizes that the phylogenetic line leading to man became independent at a relatively early date, probably no later than the end of the Oligocene; and that the stock from which it arose was far more monkey-like than anthropoid-like. He believes that the hominid line became independent of the primitive catarrhine stock not only at a pre-dryopithecine stage, but even before the differentiation of the Hylobatidae. Following a different line of reasoning, Robinson (1956), utilizing knowledge gained from his studies on the Australopithecinae, presents convincing arguments for the hypothesis of the crystallization of the hominid line from a pre-pongid prosimian complex in the Oligocene. Thus a similar hypothesis has been derived by at least three authorities from quite different facts and for different reasons.

Because of the brief nature of this article all the various contradicting and conflicting viewpoints can neither be expressed nor discussed. With this in mind and with an eye to the fact that the riddle of man's ancestry is far from solved, there is much to be said

for the alternative and sound hypothesis (Le Gros Clark, 1955) that the Hominidae and the Recent Pongidae derive from a common ancestry at least as late as Early or Middle Miocene times, and possibly even later. It was perhaps subsequent to this time, therefore, that in association with opposing trends in adaptation to posture and gait, the divergent evolutionary development of the characteristic growth rates of the limb and trunk in the Hominidae marked the initial phylogenetic separation of the earliest precursors of the Hominidae from the Pongidae. It may be implied that the line leading directly to man never passed through a brachiating phase. A rather awkward gap in the fossil record separates the commencement of the Miocene adaptative radiations from the Australopithecinae of South Africa who are probably the extinct products of a late ( ? Pliocene) radiation of one line, the surviving product of which is man (Simpson, 1952). Some of the radiations from the Miocene generalized apes were along evolutionary side-lines which petered out during the Pliocene, while others became modified and specialized to form the existing genera of anthropoid apes, of which the gibbon and orang are now confined to South-Eastern Asia, and the gorilla and chimpanzee to the tropics of Africa. One or more radiations spurted along the Hominid line by developing progressive trends towards an erect posture and perfection of the limbs for terrestrial locomotion.

A tremendous amount has been written about these ' Southern ape-men ' (for full bibliography of nearly 400 publications, see Musiker, 1954) and, whatever one accepts as their zoological status and affinities, there can be no doubt about their importance in shedding light upon the evolution of the Hominidae. Generally the controversies which have arisen since Dart's first epic paper (1925) have thrown wide open the gates for an extensive and concentrated surge of research in primate comparative anatomy to prove or disprove one or other contentious point.

The dating of the australopithecine sites in the Transvaal by climatological and faunal methods indicates an early Pleistocene period; but part of the sequence may have been terminal Pliocene, the latter period of which must have witnessed the evolutionary development of the australopithecines.

Robinson (1954, 1956) accords the Australopithecinae the status of a subfamily of the family of Hominidae, which with the Parapithecidae and Pongidae form the superfamily Hominoidea, and he believes that the Australopithecinae contain two genera, three species and four subspecies. He also refers *Meganthropus palaeojavanicus* from Java to one of the species of *Paranthropus*, and he refers *Meganthropus africanus* from the Laetotil beds of East Africa to the genus *Australopithecus* (1954, 1955, 1956). That the scanty material from both latter sites have distinct australopithecine traits is most distinct, and their inclusion is logical and indicates the widespread dispersion and evolution of this subfamily. The other subfamily that Robinson includes in the Hominidae is that of the Euhominae (or true men) which is the equivalent of the more common generic use of *Homo*.

However, there is still dispute concerning the most acceptable taxonomic status of the Australopithecinae and there are a few (for example Zuckerman, Ashton) who would not separate them from the direct anthropoid line. But from the mass of evidence available it seems clearly indicated that the Australopithecinae had already developed a considerable amount of the evolutionary pattern evolved by the true hominids after separation from the pongid baseline. Unfortunately, no true hominid fossils* have yet been recovered which

* Robinson believes (1954, 1956) that although *Telanthropus* (from the Swartkrans australopithecine deposits) is more like *Australopithecus* than *Paranthropus* it possesses considerable true hominid characteristics and had, by linking the Australopithecine and the true hominid groups, actually, or nearly, completed the transition between them. This view must yet be substantiated by more decisive and conclusive evidence than is available at present.

fall into the wide gap between the latter baseline and the australopithecine horizon. Nevertheless the Australopithecinae clearly establish the fact that locomotor (bipedal) adaptation (and not brain development) is the evolutionary factor splitting man from the apes. This places the hominoid evolutionary sequence into the same category as other mammals in accordance with Edinger's discoveries (1948).

The recent discoveries of implement-like stones and Dart's (1949, 1957) assumptions of the habits (animal-hunting, bone-breaking, etc.) of the man-apes may suggest that they were endowed not only with some of the physical distinctions of hominids but also with some behavioural adaptations. Debatable as this evidence is in the present state of our knowledge, it may yet be shown that the australopithecines (and the other distinctly ape

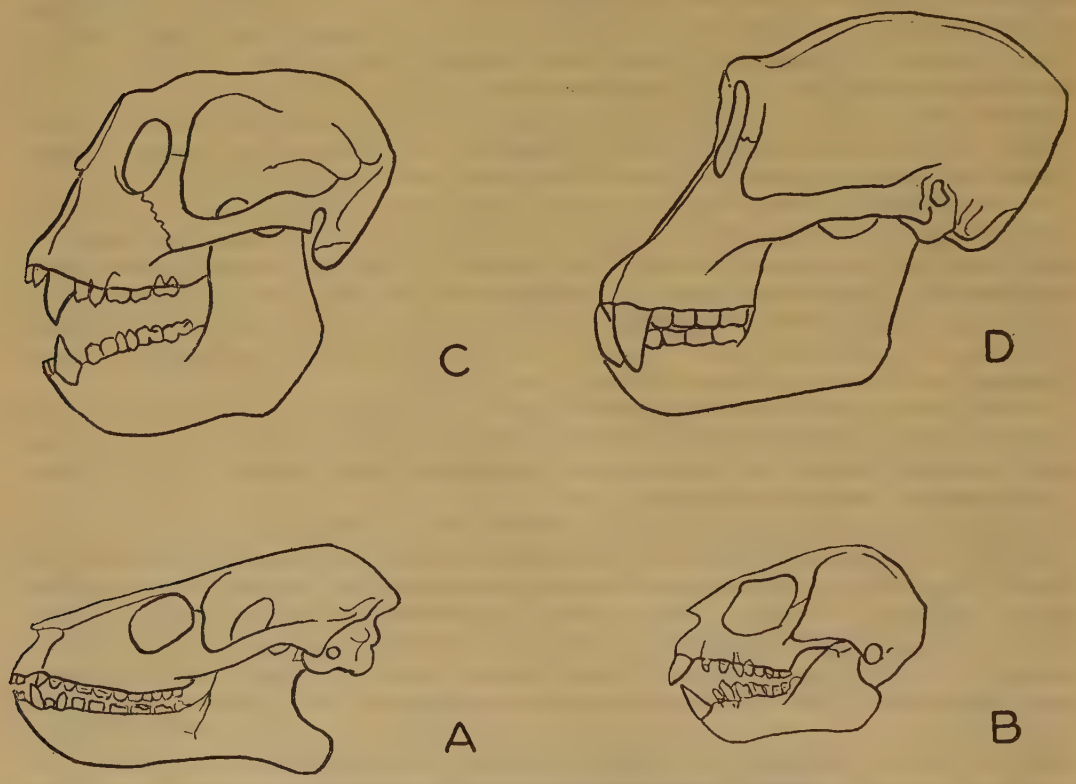



FIG. 2. SKULLS OF VARIOUS PRIMATES (AFTER COLBERT)
A. Notharctus, a fossil lemuroid. B. Tetonius, a fossil tarsioid. C. Mesopithecus, a fossil Old World monkey. D. Chimpanzee

fossil remains) were the victims of coexistent early Pleistocene ( ? Pithecanthropine-like) man, whose presence may be associated with the apparently pre-Chellean implements discovered (albeit sparse relative to the rich fossil deposits) at the Transvaal sites. This may even explain the selective extinction of the ' inferior ' ape-men. This theme is obviously open to much speculation and ' flights of fancy '.

Closely related to this pre-human australopithecine phase of hominid evolution (this does not infer that the australopithecines were directly ancestral to the human line) is the phase represented by the pithecanthropines of Java and China, *Pithecanthropus erectus* and *Sinanthropus pekinensis* respectively, or (following Mayr, 1944) *Homo erectus erectus*

(Java man) and *Homo erectus pekinensis* (Peking man). From this and preceding remarks it is obvious that dogmatic statements should be avoided concerning the interpretation of the fossil records at present, and that the anthropological nomenclature is in need of radical revision. It is commonly (not necessarily correctly) accepted that *Pithecanthropus*

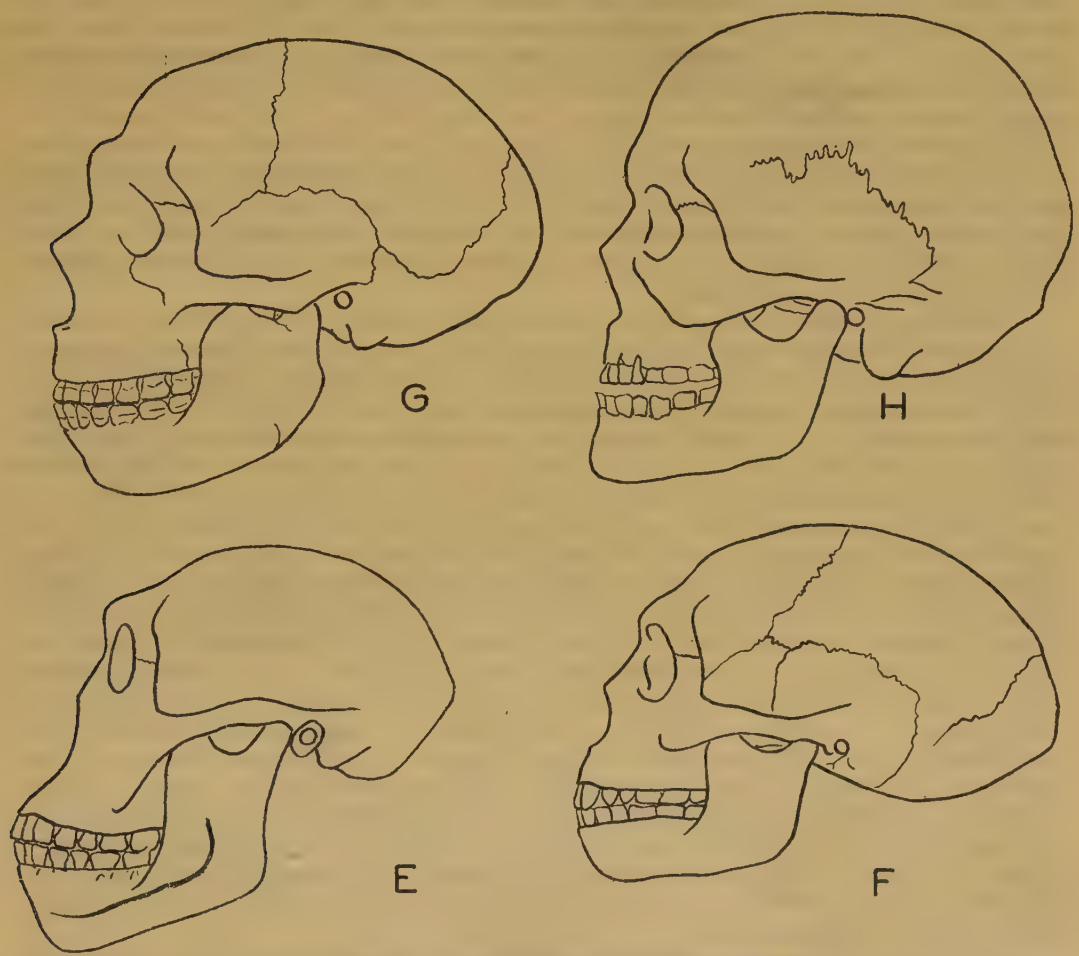



FIG. 3. SKULLS OF VARIOUS PRIMATES (AFTER COLBERT)
E. Australopithecus. F. Java man. G. Neandertal man (La Chapelle aux Saints). H. Cro-Magnon man.

is distinct from *Homo* on a generic level and that the former was probably ancestral (on the evidence available) to *Homo*. From general mammalian evolutionary trends and by extrapolation backward of the *Pithecanthropus—Homo* sequence, it would seem that groups just anterior to the *Pithecanthropus* horizon must exhibit characters very close to the simian line of evolution, and precisely this situation is found in the Australopithecinae. This does not mean that the latter gave rise to *Pithecanthropus*, but they were probably only specifically distinct from the ancestors of the latter (FIGS. 2 and 3). The primitive nature of the *Pithecanthropus* skull, brain and teeth coupled with limb bones as advanced as those of modern man even more strongly confirms the suggested status of the australo-

pithecines and also Weidenreich's (1947) suggestion (though it was only demonstrated later) that the evolution of the locomotor system preceded that of the brain and skull. The small size of the *Pithecanthropus* brain of the early Pleistocene indicates that its subsequent adjustment was a rapid process, and also that the small brain-size of the ape-men of the late Pliocene (and their terminal phase in the early Pleistocene) is such as may be expected because brains of that size may have been characteristic of apes for millions of years. The slow changes in the brain, teeth and other parts which continued into the late Pleistocene resulted from secondary selection patterns which followed after the establishment of the primary human pattern (Washburn, 1951).

The *Pithecanthropus* ape-men were culturally almost as advanced as some of the modern ' primitive ' races in that they were capable of manufacturing stone tools, apparently practised cannibalism, and allegedly knew of the use of fire. From this aspect and from the point of view of their limb bone structure allowing full bipedal locomotion, these fossils represent the early precursors of the true *Homo sapiens* horizon, and should probably be included as a species of *Homo*.

Following the *Pithecanthropus* horizon, the relatively rapid growth of the brain seems to determine subsequent forms. In the Middle Pleistocene two closely related lines arose, of which the one may have been purely an extension of the *Pithecanthropus* type (namely, *Homo neanderthalensis*) leading to an evolutionary cul-de-sac, while the other broadened into *Homo sapiens*. In the former the enlarging brain was associated with certain specializations of the skull and teeth (gross supra-orbital tori, jaws and palate) and retrogressive changes in the post-cranial skeleton. In the latter line the *more* rapidly developing brain was linked with recession of the exaggerated features of the skull and teeth which led to a higher and broader cranium, vertical forehead, and the retention or progressive ' moulding ' of limb characters for increased adaptive terrestrial function already present in the *Pithecanthropus* horizon. This is the more commonly accepted interpretation of the numerous typical Neandertal skeletal remains—' classic ' Neandertal, or Mousterian Neandertals (so-called *H. mousteriensis=H. neanderthalensis*)—which have been taxonomically separated from the more sapient specimens of earlier geological horizons—' Early Neandertals ', ' pre-Mousterian and Early Mousterian *Homo sapiens* ', ' Generalized Neandertal type ' (see Howell, 1951; Le Gros Clark, 1954; Singer, 1957). However, there are many authorities who firmly believe (as does the author) that these two groups are in effect intraracial variants, and some consider the Neandertalians (whole group) as a subspecies of *Homo sapiens* (viz., *H. sapiens neanderthalensis*), modern man being referred to as *H. s. sapiens*. In favour of the protagonists of the intraracial (or ? ecological) variants are the discoveries of Neandertal-like remains in Africa (North, Central and South), the Middle East and Java (Singer, 1954).

The biological emergence of the line leading to *Homo sapiens* from his prosimian ancestry (whether in common with the pongids in the Miocene or from a pre-dryopithecine form in the Oligocene) has taken 10 or more (even possibly 40) million struggling years. Less than one million years ago the genesis of man's culture seems to have occurred (either at the Plio-Pleistocene boundary or in the Lower Pleistocene) and more than 100,000 years ago manual tools were already expertly produced.

The adoption of bipedal terrestrial habits by the earliest representative of the Hominidae opened the way for the development of manual dexterity. Once the maximal plateau of the growth of the brain had been reached man began to adapt himself to his environment in increasing spurts. Man is not simply a clever hominoid, but a possessor of mental abilities which occur in other animals only in most rudimentary forms, if at all. The development of speech led to expression of concepts, communication of ideas and,

in a sense, to transmission of his culture. Culture is acquired and not transmitted through genes, and it can thus be communicated to any number of individuals or to future generations, regardless of biological descent or relationships, while biological heredity of populations can be changed only by the relatively slow process of breeding and selection. Thus cultural evolution is vastly more rapid and efficient than biological evolution (Dobzhansky, 1955). Consequently, man not only adapts himself to but is able to master an immense number of environments, and even actually and deliberately changes the environment. In this way he has become a biological and a geological agent. Dobzhansky states that the adaptive advantage of the ability to acquire even the most rudimentary forms of culture must have been so great in the early stages of human evolution that natural selection rapidly propagated the genotypes which permitted the acquisition of culture throughout the human species. The gene-controlled capacity to learn, absorb, and use new techniques and tools was, then, developed, intensified, and diffused by means of biological evolution, making our species more and more human.

## References


Carter, G. C. (1951). *Animal Evolution, a Study of Recent Views of its Causes.* London: Sidgwick and Jackson.

Catcheside, D. G. (1951). *The Genetics of Microörganisms.* New York: Pitman.

Clark, W. E. Le Gros (1949). *History of the Primates.* London: British Museum (Natural History).

—— (1954). ' The Antiquity of *Homo sapiens* in Particular and of the Hominidae in General.' *Science Progress*, No. 167, 377–395.

—— (1955). *The Fossil Evidence for Human Evolution.* The University of Chicago Press.

Colbert, E. H. (1955). *Evolution of the Vertebrates.* New York: John Wiley.

Dart, R. A. (1925). 'Australopithecus africanus: the Man-ape of South Africa.' *Nature*, *115*, 195–199.

—— (1949). ' The Predatory Implemental Technique of *Australopithecus*.' *Amer. J. Phys. Anthrop.*, *7*, 1–38.

—— (1957). *The Osteodontokeratic Cult of Australopithecus Prometheus.* Memoir No. 10, Transvaal Mus., Pretoria.

Dobzhansky, T. (1953). *Genetics and the Origin of Species.* New York: Columbia University Press.

—— (1955). *Evolution, Genetics and Man.* New York: John Wiley and Sons, Inc.

Edinger, T. (1948). ' Evolution of the horse brain.' *Geol. Soc. Amer. Mem. 25.*

Gregory, W. K. (1922). *The Origin and Evolution of the Human Dentition.* Baltimore: Williams and Wilkins Co.

—— (1951). *Evolution Emerging.* New York: MacMillan.

Howell, F. C. (1951). ' The Place of Neanderthal Man in Human Evolution.' *Amer. J. Phys. Anthrop.*, *9* n.s. (4), 379–415.

Hürzeler, J. (1956). *Oreopithecus, un point de repère pour l'histoire de l'humanité a l'ère tertiare. Centre National de la Recherche Scientifique, Colloque International No. 60, Avril 1955. ' Problèmes actuels de Paléontologie.'* Paris 1956.

Huxley, J., Hardy, A. C., & Ford, E. B. (1954). *Evolution as a Process.* London: George Allen & Unwin Ltd.

Mayr, E. (1942). *Systematics and the Origin of Species.* New York: Columbia University Press.

—— (1944). ' On the Concepts and Terminology of Vertical Subspecies and Species.' *Natl. Res. Council Bull. 2*, 11–16.

Musiker, R. (1954). *The Australopithecinae.* University of Cape Town School of Librarianship.

Nordenskiöld, E. (1929). *The History of Biology.* London: Paul, Trench, Trubner & Co.

Robinson, J. T. (1954). ' The Genera and Species of the Australopithecinae.' *Amer. J. Phys. Anthrop.*, *12*, 181–200.

ROBINSON, J. T. (1955). 'Further Remarks on the Relationship between "Meganthropus" and australopithecines.' *Amer. J. Phys. Anthrop.*, *13*, (3), 429–446.
—— (1956). *The Dentition of the Australopithecinae.* Memoir No. 9, Transvaal Mus., Pretoria.
ROMER, A. S. (1945). *Vertebrate Paleontology.* Chicago: University of Chicago Press.
—— (1949). *The Vertebrate Body.* Philadelphia: Saunders.
—— (1954). *Man and the Vertebrates.* Pelican Books A303, A304.
SIMPSON, G. G. (1944). *Tempo and Mode in Evolution.* New York: Columbia University Press.
—— (1952). *The Meaning of Evolution.* New Haven: Yale University Press.
—— (1953). *The Major Features of Evolution.* New York: Columbia University Press.
SINGER, C. (1949). *A History of Biology.* (p. 306). New York: Henry Schuman.
SINGER, R. (1954). 'The Saldanha Skull from Hopefield, South Africa.' *Amer. J. Phys. Anthrop.*, *12* (3), 345–362.
—— (1957). ''The Neandertal Centenary.' *S.A. Archaeol. Bull.*, in the press.
STEBBINS, G. L. (1950). *Variation and Evolution in Plants.* New York: Columbia University Press.
STRAUS, W. L. (1949). 'The Riddle of Man's Ancestry.' *Quart. Rev. Biol.*, *24* (3), 200–223.
—— (1953). 'Primates.' In *Anthropology Today*, edited by A. Kroeber. University of Chicago Press.
WASHBURN, S. L. (1951). 'The Analysis of Primate Evolution with Particular Reference to the Origin of Man.' *Cold Spring Harbor Symposia on Quantitative Biology. xv*, 67–78.
WEIDENREICH, F. (1947). *The Trend of Human Evolution. Evolution*, *1*, 221–236.
YOUNG, J. Z. (1950). *The Life of Vertebrates.* Oxford: Clarendon Press.
ZEUNER, F. E. (1950). *Dating the Past.* London: Methuen & Co. Ltd.
ZIMMERMAN, E. C. (1948). *Insects of Hawaii.* Vol. 1. Honolulu: University of Hawaii Press.

ANTIQUITY XXXI, 1957

# Tools Makyth Man

by Kenneth Oakley
*British Museum (Natural History)*

BENJAMIN FRANKLIN appears to have been the first to call Man the 'tool-making animal'[1] while Thomas Carlyle in *Sartor Resartus* (1833) declared : ' Without tools he is nothing.' In pre-Darwinian days the definition of Man was no more than a philosophical exercise. That Man might have evolved from lower animals was in the minds of very few people in those days. Certainly the question of how to draw a boundary between pre-human and human had not yet become a practical issue. Even the conception that Man had a long unrecorded past had barely taken root a hundred years ago, although the seeds of the idea had been sown by a few men far ahead of their time, such as Isaac de la Peyrère, who published a book in Paris in 1655 on *Primi Homines ante Adamum*, and John Frere whose discovery of flint tools in brickearths at Hoxne in Suffolk led him to infer in 1797 that they had been ' used by a people who had not the use of metals ', and ' belonged to a very ancient period indeed, even before that of the present world '.

The idea that Man had an extensive prehistoric past began to grow during the second quarter of the last century, largely as a result of stone tools being recognized in deposits containing the remains of extinct animals. Flint implements associated with bones of Diluvial (we should now say Glacial) animals were reported from Belgian caves in 1832, and in 1840 from below a thick layer of stalagmite in Kent's Cavern, Torquay. At this time orthodox scientists, following the French naturalist Cuvier, were incredulous of such discoveries. When, in 1846, a French customs official, Boucher de Perthes, repeated his claim (first made in 1838 ?) that he had discovered at Abbeville in the ancient gravels of the river Somme, flints worked by Man and associated with remains of Antediluvian animals, the majority of archaeologists and geologists were frankly scornful. In 1854 one of his critics, a physician named Rigollot, was converted to the unorthodox view when he himself found similar chipped flints (hand-axes) in fossil-bearing river gravel at St Acheul, near Amiens. In November 1858, the English palaeontologist Hugh Falconer visited Abbeville, and was favourably impressed by the evidence. In the following April, at Falconer's suggestion, the geologist Joseph Prestwich went to Abbeville and St Acheul. After examining the collections and visiting the pits in company with the English archaeologist John Evans, he returned to London and on 26 May, 1859, presented a paper to the Royal Society announcing his acceptance of the claims made by Boucher de Perthes and Rigollot. This pronouncement, coming from a geologist of such high repute, had a great effect on scientific opinion throughout the world. The year 1859 was, as we now see, one of the turning points in human thought : the immense antiquity of Man was established almost simultaneously with the publication in book form of Darwin's theory of evolution, *On the Origin of Species by means of Natural Selection.*

Fossilized skeletal remains of Man were found during the second half of the last century at a number of localities : Neandertal (1856), Cro-Magnon (1868), Spy (1886) and Trinil in Java (1890). It was gradually realized that these provided concrete evidence that Man had been subject to evolutionary change as deduced by Darwin in his *Descent*

[1] Quoted in Boswell's Life of Johnson, 7 April, 1778.

*of Man* (1871). However, none of these discoveries pushed back the antiquity of Man, for the unquestionable flint tools from the Abbeville gravels had already indicated his existence in Europe before the arrival of the typically glacial fauna. As Boucher de Perthes expressed it (after translation) : ' In spite of their imperfection, these rude stones prove the existence of Man as surely as a whole Louvre would have done.'

How great is the antiquity of Man ? If he is defined as the tool-making primate, this problem resolves itself into the question of the geological age of the oldest known artifacts. Although the flint hand-axes (*palaeoliths*) dating from the earlier part of the Pleistocene period are crudely flaked, they are nevertheless standardized tools, and this has generally been regarded as indicating that a long tradition of slowly acquired skill lay behind them. This was the idea sustaining the search for traces of the handiwork of Tertiary Man, in other words the hunt for *eoliths*.

I must here digress for a moment to recall that the Tertiary era, beginning 75 million years ago and distinguished by the rise of Mammals, is divided into the Eocene, Oligocene, Miocene and Pliocene periods. The Pleistocene period or Ice Age, with its post-glacial appendage of Holocene or Recent time, constitutes the Quaternary era. Originally the Pleistocene was defined as coincident with the Quaternary Ice Age, but the International Geological Congress in 1948 recommended its redefinition to include the Villafranchian stage (formerly in part Upper Pliocene) distinguished by the spread of elephants of the genus *Elephas*, oxen (*Bos*) and one-toed horses (*Equus*). Owing to these changes in nomenclature, deposits which were formerly ' Early ' Pleistocene are now counted as Middle Pleistocene (e.g. the Abbeville gravels and the Trinil beds); while other deposits formerly counted as Pliocene, i.e. Tertiary, for example the Red Crag of East Anglia, are now regarded as Lower Pleistocene by the majority of geologists. The ' glacial Pleistocene ' is estimated to have been about half a million years in duration, but the addition of the Villafranchian stage has nearly doubled the length of the period.

The problem of Eoliths, supposed artifacts of Tertiary Man, was first raised by the discoveries of Abbé Louis Bourgeois, who found in 1867 a great quantity of chipped flints, in the forms of alleged scrapers, knives and borers, in a freshwater deposit of Upper Oligocene age near Thenay (Loir-et-Cher). From time to time similar chipped flints were reported in other Tertiary formations: in Lower Eocene strata at Clermont (Oise), in Upper Oligocene gravels at Boncelles, Belgium, in ' Upper Miocene ' (now Lower Pliocene) deposits at Puy Courny, Cantal, in the so-called Pliocene plateau gravels on the North Downs in Kent, and within and below the Red Crag of East Anglia.

With the growth of knowledge on the evolution of the Primates, based on a study of actual fossil remains, it has become clear in the present century that our forerunners had only the size-range of bush-babies throughout Eocene times. Man-sized apes did not emerge before the Miocene period, while the earliest man-like remains date from early Pleistocene times. Thus, pre-Miocene eoliths at any rate may now be dismissed as geological curiosities, with no bearing on the origins of tool-making. The unreliability of the criteria used by the earlier archaeologists who recognized ' flints shaped by human agency ' in the Tertiary formations is evident from the fact that Eocene eoliths are as convincingly artificial in appearance as many of those from later formations!

In some circumstances flint is very readily fractured, and the natural agencies which cause it to be chipped into shapes reminiscent of artifice are legion. The eoliths found in Tertiary river gravels are strikingly similar to flints broken by violent agitation in the water of chalk-mills. This is understandable. An accumulation of flints of eolithic form was found in a river gravel in a valley in Würtemberg where the material carried by the main stream had evidently been drawn into whirlpools caused by the inflow of tributaries.

Another agency which results in the production of pseudo-artifacts is the pressure combined with movement occurring when flint-bearing strata founder through the solution of underlying chalk or other calcareous formation. This was the agency chiefly responsible for the Clermont eoliths. The Kent eoliths were mainly produced by the powerful friction of one stone against another which occurs in soil-creep, particularly under subglacial conditions.

While it is reasonable enough to consider the possibility of tool-using Primates having existed during Pliocene times, and even during the Miocene period, there are insuperable difficulties in the way of recognizing the particular pieces of stone or bone that may have been picked up and used. The earliest attempts at making tools from pieces of stone must have been all but indistinguishable from the accidents of nature (and we have already seen how ' human ' these can appear). Naturally fractured stones probably served as the first tools. Some Australian aborigines at the present day fashion wooden utensils with naturally shaped pieces of stone selected by virtue of their sharp cutting edges. As one French prehistorian expressed the problem of eoliths : ' Man made one, God made ten thousand—God help the Man who tries to see the one in the ten thousand.'

During the present century large numbers of flaked flints, including some strongly suggestive of intelligent design, have been found in the Stone Bed underlying the Norwich and Weybourne Crags in Norfolk and in the Bone Bed below the Red Crag in Suffolk. These flints show a number of features not found in the general run of Tertiary eoliths; and largely I believe because they date from the very interval of time when, according to many lines of evidence, tool-making probably began, they have been widely accepted as human artifacts despite the relentless opposition of scientists who have specialized in the study of flint fracture, such as Hazzledine Warren, and the late Prof. A. S. Barnes. If one confines attention to the beautiful rostro-carinates, core-like forms, scraper-like flakes, and so on, laid out for exhibition in the Norwich Castle Museum and the Ipswich Museum, it is difficult to disbelieve in their human origin. Yet, collecting for oneself in the Sub-Crag Stone Bed, for example, one is bewildered by the high percentage of the component flints that have been bruised and chipped, the majority obviously in a random fashion. Now and again the flakes have been removed first from one side and then from another side in a way suggestive of design. By keeping only the best of the latter kind one can build a collection that is convincingly like a crude human industry; yet seen as part of a vast series of flakings, with gradation from obviously natural to seemingly artificial, the Sub-Crag ' implements ' are less convincing than when studied in isolation. Nevertheless, the late Mr Reid Moir and others made a good case for accepting some of the assemblages of flakings in and below the Crag as the work of ' pre-Palaeolithic Man ', until belief in them was shaken by Barnes's demonstration that on statistical analysis the Crag flaking was of the high-angled type characteristic of the work of nature.

Out of 1800 measurements of platform-scar angles in eighteen different *human* industries, Barnes found less than 18 per cent over 90° ; but in flaked flints from Eocene deposits over 54 per cent of the flake-scars proved to have angles over 90°, while among flaked flints from the Crag formations over 62 per cent of the flake-scars are more than 90°. Prof. Barnes concluded: ' The high proportion of high-angle scars in the Tertiary flints contrasts sharply with the paucity of high angles in the human industries, and suggests that the Tertiary flaking was due to soil movements under pressure arising from solifluxion, foundering, or ice action ' (Barnes, 1943).

Warren reached a similar conclusion after prolonged study of the character of the Crag flaking (1948) and, having reviewed the geological evidence, concluded that the Stone Bed and Bone Bed did not accumulate as beach deposits, but on a submarine floor, ' which

cannot reasonably be supposed to have been available for human habitation '. As about 50 per cent of stones in some parts of the Stone Bed are flaked, and as the deposit occupies several hundred square miles, a widespread natural cause was most probably responsible for them. The striations and bruisings which many of the Sub-Crag flints show are consistent with Warren's idea that the chief cause of the flaking was the grounding of floating ice (mainly local pack-ice) which would have jammed together patches of flints

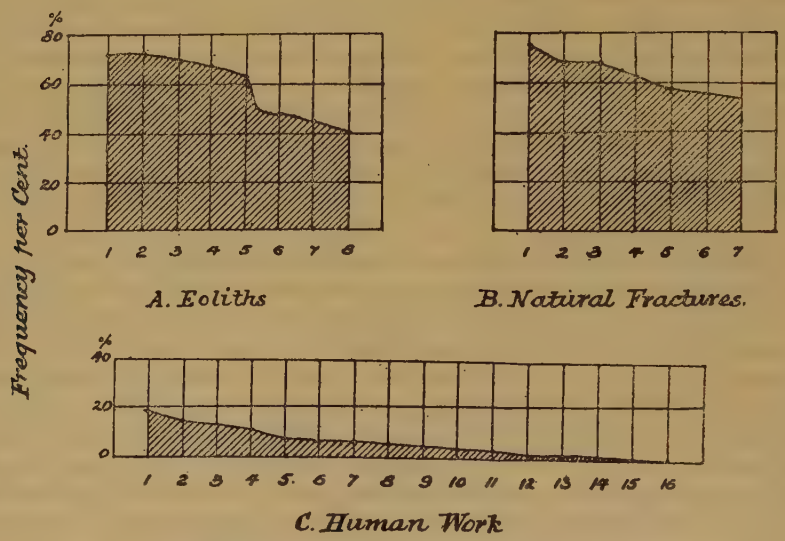



FIG. 1. PERCENTAGES OF OBTUSE PLATFORM-SCAR ANGLES FOUND IN :

(*a*) Eoliths in flint, including Sub-Crag (1 and 3).
(*b*) Naturally fractured stones, including flints crushed under cart-wheels (3) or broken in foundering of deposits (4), and chipped hard rocks in moraines (7).
(*c*) Human flint industries, mainly early, including Abbevillian (2), Acheulian (5–8, 12), Tayacian (9, 10) and Mousterian (13).
(After A. S. Barnes, by courtesy of the Editor of *American Anthropologist*.)

strewn on the floor of the shallow sea (Warren 1948). Certain flakings strewn on an alleged land-surface within the Red Crag at Foxhall have been widely accepted as artifacts, even by those who do not accept the Sub-Crag chippings. To my mind the Foxhall ' floor ' is an enigma to be further investigated, rather than a proof of the existence of Man in Britain during the Villafranchian stage.

If the Tertiary and Sub-Crag eoliths are unacceptable, what then are the oldest undoubted artifacts ? Certainly not those of the ' Cromerian industry ' which has proved to be non-existent, consisting apparently of flakings produced on the Cromer foreshore in recent times by the concussion of one stone against another in storms. These flakings occurred among a spread of big stones which had the appearance of passing under the Forest Bed (Warren 1939). The only flaked flints found *in situ* at this site appear to have belonged to the Stone Bed of the underlying Weybourne Crag. The ochreous patina of the foreshore flakes was at one time regarded as proof of their antiquity, but Warren found evidence that this patination is acquired in a few years on the Cromer coast, possibly on account of the action of some alga.

Recently Dr Alfred Rust (1956) published an account of some sandstone pebbles with chipped margins found in the Mauer Sands of earliest Middle Pleistocene age which

yielded the famous mandible of ' *Homo* ' *heidelbergensis*.[2] He kindly gave me the opportunity to examine his collection last year ; but I must confess that I was unable to find any among those from the Mauer Sands which I could unreservedly accept as possibly worked by man. Most of the local sandstone, even before it was weathered, is too coarse and friable to make serviceable tools. Indeed, Heidelberg Man could have cut more effectively with flakes of the hard limestone (Muschelkalk) available in the same valley.

In Europe we are left with the Abbevillian hand-axes in the 40-metre terrace of the Somme as the oldest unquestionable artifacts. The only question here is whether the geological horizon of the hand-axes is First Interglacial or an interstadial within the Second Glaciation, which is by no means impossible. (In fact, the same question applies to the Mauer Sands.)

When we turn to Asia we find that the oldest undoubted artifacts are in the same time bracket, either immediately antedating or contemporary with some phase of the Second Glaciation. The Pre-Soan flakes from the Boulder Conglomerate in the Punjab are not entirely convincing, for they may have been produced by glacial action ; but there is no doubt about the chert chopping tool of Early Soan type found at Locality 13, Choukoutien (China), in association with fauna indicating a stratigraphic horizon within the period of the Second or Mindel Glaciation. The slightly later flake-and-chopper-tool industry associated with Peking Man at Locality 1, Choukoutien, is now generally regarded as dating from the beginning of the Second Interglacial period. Some authors still claim that it is older, either inter-Mindel or even Günz-Mindel, but the associated fauna is clearly Middle Pleistocene in the modern sense.

In Africa, particularly south of the Sahara, high-level river gravels of Lower Pleistocene age have been found to contain large numbers of chipped pebbles, mainly of quartzite, which have been widely accepted as artifacts. These so-called pebble-tools were first recognized in Uganda by the geologist E. J. Wayland in 1920, who referred them to a pre-Palaeolithic stage of culture, later named Kafuan on the basis of their typical occurrence on the 270-foot terrace of the Kafu River. In a recent monograph on the prehistory of Uganda, the late Professor van Riet Lowe described no less than twenty-seven constantly recurring forms of ' Kafuan-type split and trimmed pebbles ', and claimed that he and Wayland could trace the evolution of rostro-carinates from the simple split pebble, thus providing a link with the discoveries in East Anglia, where Moir found a gradation from rostro-carinates to Chellean (Abbevillian) hand-axes. Kafuan-type pebbles are even found in a layer of lateritic ironstone at the base of the 270-foot Kafu terrace which is either Lower Villafranchian or Pliocene. If there were no question about their being artifacts, these chipped pebbles would be the oldest evidence of a tool-making hominid and would support the idea of an almost infinitely slow evolution of the earliest culture over some half a million years, from the end of the Pliocene to the first hand-axes.

As doubts grew about the artificiality of chipped flints in the Tertiary and Villafranchian formations of Europe, belief waxed strongly in favour of the African pebble-tools, because these were not in flint (which breaks so easily under a variety of forces), but in quartzite and the like. Moreover, they were for the most part in situations far removed from Pleistocene glaciers and pounding sea-waves which have produced such quantities of pseudo-artifacts in Europe.

Some prehistorians are now beginning to feel that the Kafuan pebbles[3] have been

---

[2] Now regarded as the European equivalent of *Pithecanthropus*.

[3] Excluding the ' advanced Kafuan ' of some authors, which is synonymous with the unquestionably human Early Oldowan.

accepted as artifacts without sufficient consideration being given to the possibility that these also were fractured by natural forces. Should not quartzite gravels of, say Triassic age be searched for similar forms, to see if they are produced in circumstances where no question of tool-making arises? There is a great need, too, for experimental work on the lines of that by the late Professor Barnes and Mr Warren, but designed to investigate the forces required to split quartzite pebbles. My own extremely limited observations in Africa suggest that at least some Kafuan-like flakings are produced when gravel is carried over waterfalls. As for secondary chipping, it should not be forgotten that soil-creep or solifluxion can occur under tropical conditions as well as under glacial.

One of my reasons for being sceptical about the Kafuan ' pebble-tools ' is their prodigality. In the summer of 1955 I was with a party on safari through the Katanga Province of the Belgian Congo, and we were taken by Professor G. Mortelmans to an exposure of gravels at Kafila near Elisabethville. Within a few minutes all the members of the party had found more ' pebble-tools ' in this gravel than they could carry away. It is true that artifacts are astonishingly abundant at some African localities on the later hand-axe horizons, but the profusion of chipped pebbles in the Kafilan gravel reminded me of the profusion of eoliths in the Sub-Crag Stone Bed. Mr Miles Burkitt evidently has similar reservations to make about the Kafuan pebbles, for he wrote recently: ' From a study of the objects themselves it is not easy to find enough evidence to prove that they *must* be the handiwork of Man and that the fracturing cannot have been produced by natural forces.' There has been an undeniable tendency among us all to argue subconsciously like this: ' Man *could* have made these; they are of the right age for the beginning of culture; this is where we may expect to find artifacts; these *are* artifacts.'

Fortunately there are certain situations in which fractured stones are acceptable as artifacts, even when the flaking appears random or accidental. For example, many of the fragments of quartz in the Choukoutien cave deposits would not be recognized as humanly struck if they were found on the surface away from their human context. The quartz found at Choukoutien is foreign to the site, and can only have been introduced into the cave by human agency. Another situation in which flaked stones are more readily acceptable as artifacts is where they occur isolated in an otherwise stoneless layer of lacustrine mud or sand.

If we discount the Kafuan pebbles, the oldest undoubted artifacts in the world are related to or equivalent to the Oldowan culture, first recognized by Dr L. S. B. Leakey in the basal bed of the series of lacustrine sediments exposed in the side of the Oldoway (Olduvai) Gorge in Northern Tanganyika. The associated fauna indicates that Bed I of Oldoway is of early Middle Pleistocene age. The typical Oldowan industry found in this bed consists of pebbles or other lumps of rock flaked by percussion to form crude chopping tools ' varying in size from the dimensions of a ping-pong ball to that of a croquet ball '. The chopping edges were made by removing flakes in two directions along one side of the pebble or lump, so that the intersecting flake-scars formed a jagged cutting edge. The fact that occasionally these pebble-choppers foreshadow bifacial hand-axes, and the fact that hand-axes appear and begin to replace pebble-tools in Bed II, are indications that the Abbevillian-Acheulian sequence of cultures evolved from the Oldowan. The discovery of a few apparently Oldowan-type pebbles in the Laetolil Beds of Tanganyika and in the Kanam Beds of Kenya suggested that the beginnings of Oldowan culture were to be sought in the Lower Pleistocene (Villafranchian) beds of Africa, but until quite recently there has been an element of uncertainty about all pebble-tools from Villafranchian deposits.

Unquestionable stone industries closely comparable with the Oldowan have now been

found in North Africa and in South Africa in fossil-bearing deposits of Late Villafranchian age. The Northern occurrence was discovered by Professor C. Arambourg in lake-margin deposits at Ain Hanech in Algeria, and consisted of quantities of pebbles or stone lumps flaked in several directions to form sub-angular stone balls that would have served equally well as missile stones or multi-edged pounders. A few hand-axes of Abbevillian type are reported as coming from an immediately overlying layer. These Algerian industries are clearly the homotaxial equivalents of those in Beds I and II at Olduvai.

The discovery in March 1953 of pebble-tools of ' advanced Kafuan ' (I would now say ' Early Oldowan ') type in the calcified surface of the Basal Older Gravels in the 200-foot terrace of the Vaal Valley in South Africa was the first positive indication that tool-makers existed in South Africa as early as the dry phase which terminated the First (or Villafranchian) Pluvial period. The discovery raised a most intriguing problem, because cave deposits that accumulated during this dry Late Villafranchian stage at Taung in Bechuanaland and at Sterkfontein and Makapan in the Transvaal had yielded remains of the subhuman *Australopithecus*. Was it possible that the pebble-tools had been made by that creature ? Analysing the evidence available in 1954 I argued that it was possible, but I thought unlikely for two reasons. In the first place, although the Australopithecines walked upright on two legs and qualified structurally to be regarded as Hominidae, rather than as true apes or Pongidae, nevertheless in absolute size their brains were on an average no larger than those of apes. All the known tool-making hominids had brains considerably larger than those of apes. The earliest known tool-makers, the Chinese representatives of the genus *Pithecanthropus*, had skulls with average capacity about twice as great as that of *Australopithecus*. In the second place, Professor von Koenigswald had reported remains of an early *Pithecanthropus* in the Djetis Beds of Java which he claimed were of Villafranchian age. In other words, *Australopithecus* and *Pithecanthropus* evidently existed in the world contemporaneously, and at Peking at least there was evidence that the latter was capable of tool-making. Thus it seemed most probable on the evidence available in 1954 that the pebble-tools in the Vaal valley terrace had been made by true men, of the *Pithecanthropus* group, who had penetrated into South Africa before their more backward relatives the Austalopithecines had died out. The above hypothesis seemed to be strongly supported by the absence of pebble-tools from all the sites where remains of Australopithecines had been discovered, suggesting that although pebble-tool makers existed contemporaneously with the Australopithecines, the two groups frequented different environments.

The whole picture has now been altered by further discoveries. Two years ago, some possible pebble-tools, mostly of dolomite, were found in the gravel bed overlying the main Australopithecine deposit in the Limeworks Cave at Makapan in the Transvaal. These were commented on in ANTIQUITY, XXX (March 1956) p. 6, and one of the chipped pebbles was illustrated (pl. II). There was considerable doubt as to whether these were artifacts, and the general opinion was that unless more convincing specimens came to light in the same bed they were better discounted as evidence bearing on the cultural status of *Australopithecus*.

In May 1956 Dr C. K. Brain discovered indubitable pebble-tools of Oldowan type in the upper part of the Australopithecine breccias at Sterkfontein in S. Transvaal. This is possibly the most important discovery in the field of palaeoanthropology since the finding of implements with Peking Man. Excavations at Sterkfontein carried out this year by Dr J. T. Robinson and Mr Revil Mason have confirmed beyond all doubt that the artifacts observed by Brain are part of an industry occurring *in situ* in a layer of breccia containing teeth of *Australopithecus*.

It is worth reiterating that the hypothetical attribution of the pebble-tools, not to Australopithecines themselves, but to some higher type of hominid living contemporaneously with *Australopithecus* in the Transvaal has rather depended on the assumption that the two types would have occupied different ecological niches. As Bartholomew and Birdsell (1953) pointed out: ' By analogy with the ecology of other animals it would be surprising if man and the Australopithecines[4] had remained contemporaries in the same area over very long periods of time, for closely related forms with similar requirements rarely occupy the same area simultaneously.'

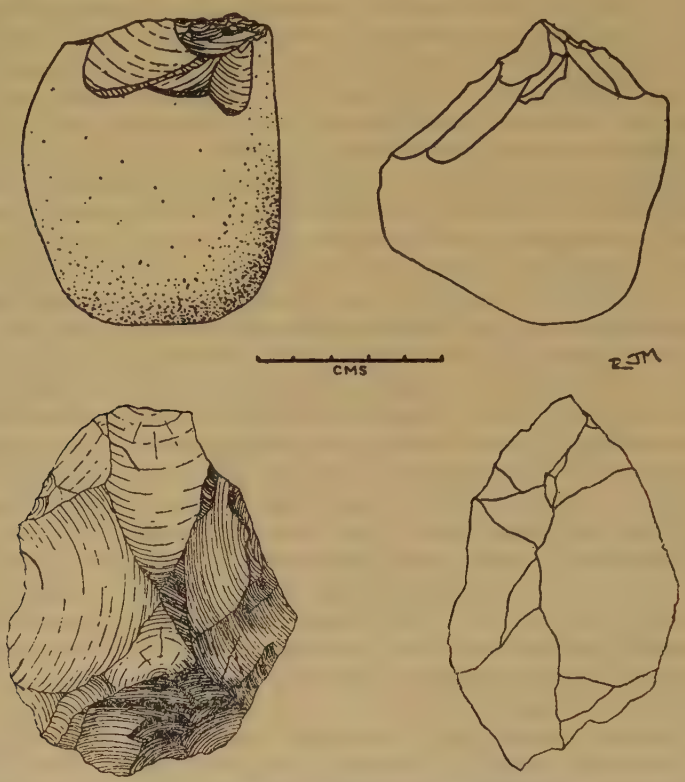



FIG. 2. TWO OF THE 58 STONE ARTIFACTS FOUND IN LAYER OF RED-BROWN BRECCIA CONTAINING TEETH OF *AUSTRALOPITHECUS* AT STERKFONTEIN, TRANSVAAL.

(After R. J. Mason, Archaeological Survey of S. Africa, by courtesy of the Editors of *Nature*.)

*Above:* Pebble-tool of Oldowan type, in diabase, described as utilized hammerstone.
*Below:* Bifacially-flaked core in quartzite, possibly used as chopper ; prototype of hand-axe.
(It is noteworthy that diabase and quartzite are foreign to the site, and therefore were carried there.)

Brain's discovery has now shown that pebble-tools were made at the very site where *Australopithecus* occurred.

At the present time there is general agreement among palaeontologists that the Australopithecine breccia at Sterkfontein is of Late Villafranchian (Lower Pleistocene) age. On the other hand Dr D. A. Hooijer's further analysis of the fauna of the Djetis

[4] The authors were assuming that the Australopithecines were *non*-toolmakers, in contradistinction to Man.

Beds in Java, which contained the remains of the earliest known examples of *Pithecanthropus*, has indicated that they are probably early Middle Pleistocene and not Villafranchian as previously claimed by von Koenigswald. While the last word on this question may not yet have been said, it is nevertheless true to say that there is no undisputed evidence that any hominids higher in type than *Australopithecus* were in existence in the world at the time when the Sterkfontein tools were manufactured.[5]

So it is only our belief that systematic tool-making requires a larger brain than the ape-size brain of *Australopithecus* (a belief which may prove to be ill-founded), that makes us hesitate to infer that the Sterkfontein tools were probably made by that creature.

Another reason for doubting that the tools were made by *Australopithecus* is the fact that ' they have no background ' at this or any other Australopithecine site. No pebble-tool or any kind of stone artifact has been observed in the series of underlying layers at Sterkfontein which yielded relatively abundant Australopithecine skeletal remains (traces of at least sixteen individuals, including the skull known as Sterkfontein 5 and the associated spinal column and pelvic girdle). Robinson has suggested that the absence of pebble-tools from the underlying layers is against *Australopithecus* being a tool-maker ' since on all other Stone Age sites remains of the tool-manufacturer are extremely rare '. I believe the relevant point is that, cannibalism aside, Primates do not as a rule die or leave the remains of their dead at the living place (Oakley 1954*b*, p. 66). In other words the substantial quantity of Australopithecine skeletal remains in the main bone-bearing breccia at Sterkfontein is consistent with the late Dr Broom's suggestion that the cave was originally a carnivores' den rather than the actual living-place of the hominid.[6] The red-brown breccia containing the stone tools (and a few isolated teeth of *Australopithecus* and fragmentary animal remains) was, on the other hand, accumulated at a time when the cave had temporarily become a site of hominid occupation.

There are many related issues to be considered in the light of the new evidence from Sterkfontein. For example, in the breccia of the nearby site of Swartkrans, accumulated at a later date, numerous remains of an aberrant Australopithecine named *Paranthropus* have been found, also some fragments, including a lower jaw, referred to a related form called ' *Telanthropus* ', which Robinson believes may have been the tool-maker. However, as the Swartkrans breccia is on a later time horizon than the Sterkfontein tools, one feels hesitant in attempting to explain the latter in terms of the former.

If in fact *Australopithecus* was the maker of the Sterkfontein tools, it would involve almost a revolution in our conception of ' Man '. We have already seen that there is some doubt about the existence of ' Kafuan culture '. That is to say, there is no reliable evidence of tool-making before the Late Villafranchian time level. Is it possible that systematic tool-making arose, not gradually as most 19th century evolutionists led us to imagine, but suddenly and spread rapidly ?

The Australopithecines must have originated as apes that became adapted to life in open country by walking upright. There are many reasons to suppose, as Dart, Bartholemew and others have shown, that the earliest hominids must have been tool-*users*. Bipedalism is initially disadvantageous biologically unless there is some compensating

[5] The antiquity of the fragmentary mandible of *Homo* cf. *sapiens* recorded from the Kanam Beds of Kenya is now considered too doubtful to stand in the way of this generalization.

[6] The evidence collected in South Africa recently indicating that modern hyaenas do not carry carcases or bones into caves (Dart, 1957) has little bearing on the activities of extinct species or varieties of hyaena. Behaviour of mammals is modified in response to environmental, particularly biotopic, changes. One species of Macaque, for instance, seeks and eats crabs, but one would be wrong to suppose that this habit is characteristic of all species of *Macaca*.

factor—in the case of the hominids this was the ability to use tools and wield weapons while moving. The early hominids survived in open country by becoming scavengers and hunters, and this they were enabled to do by developing ' extra-corporeal limbs ' (as O. G. S. Crawford once called tools and weapons), which could be changed or discarded as circumstances dictated. The earliest tools and weapons would have been improvisations with whatever lay ready to hand. Although the hominids must have begun as occasional tool-users, ultimately they were only able to survive in the face of rigorous natural selection by developing a system of communication among themselves which enabled cultural tradition to take the place of heredity. At this point systematic tool-making replaced casual tool-using, and it may be that this change-over took place in the Australopithecine stage.

It would not be surprising, in view of the close correlation between culture and cerebral development, if there had been at this stage intense selection in favour of larger brains, with the result that the transition from the small-brained *Australopithecus* to the larger-brained *Pithecanthropus* took place in a comparatively short space of time. The discoveries at Sterkfontein suggest that pebble-tools may have been made by *Australopithecus*, while Prof. Arambourg's finds at Ternifine (' *Atlanthropus* ') indicate that by the time culture in Africa had reached the beginning of the Acheulian stage the tool-makers had attained the grade of *Pithecanthropus*.

## References


Arambourg, C. (1950). ' Traces possible d'une Industrie primitive dans un niveau Villafranchien de l'Afrique du Nord ', *Bull. Soc. préhist. Franç.*, vol. 47, pp. 348–50.

—— (1955). ' A recent discovery in human palaeontology, Atlanthropus of Ternifine (Algeria) ', *Am. J. Phy. Anthrop.*, N.S., vol. 13 (2), pp. 191–201.

Balout, L. (1955). *Préhistoire de l'Afrique du Nord*, Paris. (Ain Hanech, pp. 163 ff.)

Barnes, A. S. (1939). ' The Difference between Natural and Human Flaking on Prehistoric Stone Implements ', *Amer. Anthrop.*, vol. 41, pp. 99–112.

—— (1943). ' High-Angle Edge Flaking of Flint ', *Nature*, vol. 152, p. 477.

Bartholomew, G. A., and Birdsell, J. B. (1953). ' Ecology and the Protohominids ', *Amer. Anthrop.*, vol. 55, no. 4, pp. 481–98.

Black, D., Teilhard de Chardin, P., Young, C. C., and Pei, W. C. (1933). ' Fossil Man in China : The Choukoutien Cave Deposits with a Synopsis of our Present Knowledge of the Late Cenozoic in China ', *Mem. Geol. Surv. China*, ser. A., no. 11.

Broom, R. (1946). ' The Occurrence and General Structure of the South African Ape-men ', *Transvaal Museum Memoir*, no. 2, pp. 7–153.

Burkitt, M. C. (1956). ' Some Early Prehistoric Cultures South of the Sahara ', *Scientia*, vol. 91, no. 10, pp. 304–8 (esp. p. 306).

Crawford, O. G. S. (1921). *Man and his Past*, Oxford.

Dart, R. A. (1957). ' The Osteodontokeratic Culture of *Australopithecus prometheus* ', *Transvaal Museum Memoir*, no. 10.

Hooijer, D. A. (1957). ' The Correlation of Fossil Mammalian Faunas and the Plio-Pleistocene Boundary in Java ', *Proc. Kon. Ned. Akad., Amsterdam*, ser.B., vol. 60, no. 1.

Koenigswald, G. H. R. von (1950). ' Fossil Hominids from the Lower Pleistocene of Java ', *Rep.XVIII Int. Geol. Cong.* (1948), pp. 59–61.

Leakey, L. S. B. (1951). *Olduvai Gorge*, Cambridge.

Lowe, C. van Riet (1952). ' The Pleistocene Geology and Prehistory of Uganda ', Part II, *Geol. Surv. Uganda*, mem. no. VI.

Moir, J. R. (1927). *The Antiquity of Man in East Anglia*, Cambridge.

Movius, H. L. (1949). ' The Lower Palaeolithic Cultures of Southern and Eastern Asia ', *Trans. Amer. Phil. Soc.*, N.S., vol. 38, pt. 4. (Pre-Soan, p. 377, Locality 13, p. 390.)

OAKLEY, K. P. (1954*a*). ' The Dating of the Australopithecinae of Africa ', *Am. J. Phy. Anthrop.*, N.S., vol. 12, pp. 9–28.
—— (1954*b*). ' Early Man in Africa ', *S. Afr. Arch. Bull.*, vol. 9, pp. 64–68.
—— (1949). *Man the Tool-Maker*, Brit. Mus. (Nat. Hist.), London (3rd edn. 1956).
OBERMAIER, H. (1924). *Fossil Man in Spain*, New Haven. (Good account of eolith controversy.)
PEI, W. C. (1934). Report on The Excavation of the Locality 13 in Choukoutien, *Bull. Geol. Soc. China*, vol. 13, no. 3, pp. 359–67.
ROBINSON, J. T., and MASON, R. J. (1957). ' Occurrence of Stone Artifacts with Australopithecus at Sterkfontein ', *Nature*, vol. 180, pp. 521–4.
RUST, A. (1956). *Artefakte aus der Zeit des Homo heidelbergensis in Süd- und Norddeutschland*, Bonn.
WARREN, S. H. (1940). ' Geological and Prehistoric Traps ', *Essex Nat.*, vol. 27, pp. 13–19.
—— (1948). ' The Crag Platform : its Geology and Archaeological Problem ', *S.E. Nat. and Antiq.*, vol. 53, pp. 48–52.

ANTIQUITY XXXI, 1957

# The Evolution of Society

by V. Gordon Childe

WITH the general acceptance of the doctrine of organic evolution continuity between human history and natural history was also accepted. The latter became just the latest chapters in a single historical record with archaeology bridging the gap between the record of the rocks and the written record. The content of these latest chapters may be termed social evolution, and the Darwinian mechanisms of variation, adaptation, selection and survival may be invoked to elucidate the history of man as well as that of other organisms. But while the use of these terms may emphasize the continuity of history, it may also cause confusions and, in fact, misled some early anthropologists and archaeologists when they tried uncritically to apply Darwinian formulae to human societies or artifacts.

Zoologists write of the evolution of a genus, say the *Equidae*, and of an organ, say the leg, but use the word in a different sense in each case. Similar divergences will infect the word if we apply it to societies or Society, to kingship or ' the axe '. The leg of *Equus cabalius* can be called fitter than the leg of *Equus stenonis*, but only because *Equus cabalius* as a whole organism is fitter than *Equus stenonis*. On the other hand the socketed axe may be called fitter than the flat axe not because its human users were fitter than those who used flat axes but because the same men found the socketed axe the fitter instrument for the same purpose. The word ' evolution ' can be legitimately applied to four distinguishable processes because all four processes have at least one element in common. The word implies the affirmation that the processes thus designated are intelligible to human reason and amenable to empirical description without reference to interventions by unknowable or unpredictable forces outside the processes themselves. The doctrine of organic evolution as enunciated by Lamarck in 1800 was an alternative to the accepted account of the origin of species through a series of distinct divine interpositions. Darwin's provision of convincing mechanisms was followed by the abandonment of the older doctrine in Natural History. That is what evolution primarily means for human history too. If an historian writes of the evolution of kingship, he thereby proclaims that he does not assume that ' kingship descended from the heavens ', as the Sumerian annalists put it.

So too with the Darwinian mechanisms. In ' natural selection ' the emphasis is on the first word, not the second. As in ' natural history ' it means exemplifying laws that can only be discovered inductively by observations on the process; for they are not imposed upon it. The selecting is not done by some agency outside the process. The development of mathematical probability theory in the hands of geneticists like Haldane has revealed the mechanism for producing from random variations the enormously improbable order presented by organic nature. But in the case of institutions and artifacts the selectors are men and the agency human volitions. When we still speak of their ' evolution ', we admit that human will is natural too.

' Survival of the fittest ' would be a tautology if it were known in advance by some mystic revelation what ' fitness ' means. The business of an evolutionary biologist or historian is not to predict what will survive because it is ' fit ', but to discover empirically what has survived and then by closer observation to make plain in what this fitness consists. The rank of an organism or artifact in an evolutionary hierarchy can be established only

by the place it maintains in a chronological order concretely disclosed in stratified rocks, superimposed settlements or written texts.

The variations upon which selection operates equally naturally in natural and human history none the less arise and spread in different ways in the two divisions. In the earlier chapters variations result from gene mutations that are handed on from parents to offspring over successive generations. In the latest chapters random mutations have been replaced by discoveries and inventions, in a word by new ideas arising in the heads of innovators by psychological, and sometimes at least rational, processes. They are transmissable not by copulation but by education, not only to their author's direct descendants in the next generation, but to all his contemporaries and fellows. Thereby the selective process may be so much accelerated that its very character is changed. It takes a large number of generations and, in the case of slow-breeding organisms like men a number of centuries, for an advantageous mutation to spread through a population. New ideas could potentially thus spread in fewer hours throughout a single society. A new mechanical invention could become established with as much speed as a new strain of influenza virus or myxomitosis bacilli!

Finally if among human societies, as among animal species, survival depends on adaptation to the environment, it is effected in different ways among men and among other organisms. Among the latter, adaptation to a different environment is achieved by changes in bodily organs, resulting, like other variations, from mutations of genes and transmissable only in the same biological way. Among men the same result is achieved by extracorporeal organs, by artifacts—clothes, houses, hypocausts, air-conditioning plants. They embody discoveries and inventions and are transmitted and diffused in the same way to contemporaries as well as descendants by precept and example.

Unless the foregoing differences be duly appreciated, unless logical deduction be submitted to the control of empirical induction, the application of evolutionary terms and concepts to human history may do more harm than good. Last century Herbert Spencer, Lewis H. Morgan, and Tylor propounded divergent schemes purporting to depict the unilineal evolution of human society or of human institutions. As a result of comparative studies of existing contemporary human societies or institutions they arranged these in a logical order, indeed in a hierarchy. They assumed that the logical order was also a temporal one, that the hierarchy was a historical sequence. Of course Linnaeus' System of Nature itself was just such a logical construct, based upon comparisons between existing orders, genera and species. But long before 1857 stratigraphical geology had provided objective proofs that the logical order did correspond to an actual chronological sequence; the orders, genera, etc. that rank as ' higher ' in the hierarchy actually first appear higher, i.e., later, in the undisturbed succession of fossiliferous beds. A century ago the archaeological record could not perform the same service for any arrangement of human societies. Nor for that matter can it now. Unfortunately those aspects of human behaviour upon which social evolutionists normally focused attention—kinship systems, institutionalized sexual relations, forms of government—do not normally fossilize to leave a fleshless imprint on the rubbish deposited in superimposed layers by preliterate men. The competing systems of Morgan and Tylor remain equally unverified—and incompatible—theories.

Similar comparative studies on collections of artifacts led to the construction of typologies or hierarchical series that should illustrate the—logical—evolution of ' the plough ' or ' the axe ' and so on. In so far as the artifacts serialized are durable—ploughs unfortunately do not survive in sufficient numbers to warrant any confident conclusion—such typologies are amenable to the test of archaeological observation. In the established frame of relative chronology do the several variations occur in the order demanded by the

theory? Some beautiful theories have thus been disproved to survive merely as warnings that armchair archaeologists and ethnographers have no esoteric knowledge of what practical man did find the fittest instrument for cleaving logs or skulls, for tilling a forest clearing or what you will. Åberg[1] once suggested a plausible and ingenious typological series for deriving the symmetrically-bladed battle-axe from a mace-head of the Crichie type. Unfortunately the historical order was just the reverse; his starting point is Late Bronze Age, the last derivative battle-axe belongs to the Beaker phase!

On the other hand the establishment of reliable relative chronologies has quite often justified evolutionary series propounded before stratigraphical evidence was available. The socketed celt is everywhere later than the flat or flanged celt and established its superior fitness by superseding these. But even here refinements in chronology have tended to discredit the old typologies in so far as these interposed winged axes or palstaves as intermediate stages between the flat axe and the socketed type. Indeed it looks quite possible today that the socketed celt was evolved by divergent routes and in more than one place, perhaps for example in Europe and in China. Moreover, in Hungary and Eastern Europe the socketed bronze axe superseded not only the flat axe, but also the shaft-hole axe. It thereby established its superior fitness to both forms. Now this result no theory anticipated. Obviously a socketed axe can be attached to its handle much more securely than a flat axe or even a palstav. But in this respect it is no fitter than a shaft-hole axe. So prehistorians, having established the fact of superior fitness, were forced to discover by closer observation in what this fitness consists. The answer[2] turned out to be cost. A socketed axe contains about half as much metal (by weight) as a shaft-hole axe of equal cutting edge or a palstav and considerably less than even a flat axe! Of course with the introduction of cheap iron as the industrial metal, the socketed axe lost much of this advantage; being rather harder to forge than a shaft-hole axe, it gradually became obsolete.

Even where, as in the foregoing illustration, the evolutionary fitness of a tool can legitimately be equated with its efficiency, its survival cannot in practice be predicted from the data available to the historian, even if aided by written records. For efficiency is not a simple function of the time consumed in performing a specific operation with the instrument; the time required for making the tool and making it available where it is to be applied, must also be reckoned in. The total situation is involved, and it is too complex for computation.

If the fruitful application of evolutionary terms to an artifact, an institution or Society demands caution, they can be applied to societies only with more cautious reservations. For now societies are liable to be regarded as mutually exclusive units, like genera or species, whereas hitherto Society has been treated as an institution or at best the totality of institutions, and the analogy in natural history has been a limb or an organ. The criteria of ' fitness ' have been intrinsic, not competitive; flat axe and socketed axe are not exclusive alternatives. But in Darwinian theory species were exclusive and established their fitness by success in a struggle for existence, for a supposedly limited supply of food and other necessities. The variations by which survival and adaptation were achieved were specific, confined to the members of the species, but the advantageous variations which have been decisive in human history are artifacts, that is, realized ideas, and ideas can and do leap political and even linguistic frontiers. They are transmitted by example

---

[1] Åberg, *Das nordische Kultur-gebiet Mitteleuropa während der jungeren Steinzeit*, Uppsala, 1918, I, pp. 23 ff.

[2] Childe ' The Socketed Celt in Upper Eurasia,' Univ. of London, *Ins. Arch. Tenth Annual Report* (1954), pp. 11 ff.

as well as by precept. Even the weapons of offence and defence, by which in the last resort one society might vindicate its superior fitness over another, are sooner or later successfully imitated by the enemy. If this has proved true even of the immensely complicated engines employed by civilized powers, it was much more true of the simpler devices of barbarian warfare; slings and multiple ramparts were presumably introduced into Britain by a single band of invaders, but were soon adopted by all native tribes. Moreover, in human history even means of subsistence have not remained rigidly fixed; the neolithic revolution, the replacement of shifting cultivation by systematic rotation, and subsequent advances, each meant absolute increases in the food supply.

Sir Arthur Keith[3] was of course right in emphasizing the value of psychological qualities like loyalty, ' clannishness ', etc., in a competitive struggle for survival. No doubt too such qualities, though ideal, are less easily transmitted by example than the ideas that are materialized in artifacts. They are habits, observable as traditional behaviour patterns. Yet it is far from certain that such patterns are genetically determined. The respective roles of nature and nurture in these domains of human behaviour are still highly controversial issues on which the competent experts are far from unanimous. Obviously materialized ideas of a kind that can be and have been transmitted from one society to another impose new patterns of behaviour, new habits, we might call them new ' psychological dispositions ', on the recipient society. We have seen during the last half-century how mechanization has compelled peasants to adopt quite alien habits of punctuality and precision, novel attitudes towards time and personal hygiene. In Russia it took a whole generation, but that is a tiny fraction of the time that would be needed to establish a favourable mutation by biological processes in a population of that size.

In fact the intrusion of diffusion as an agent of evolution, not of course as an alternative thereto,[4] has so drastically altered the historical process that no analogy between the evolution of species and the evolution of societies is valid. Even prehistory deals with societies that were seldom, if ever, genetically homogeneous; the cultures that represent them in archaeology never demonstrably stand for pure races, assemblages of genotypes, that can be regarded as embryo species. Indeed, Sir Arthur Keith himself tended to limit the effective operation of biological selection to the earliest chapters of prehistory. With the adoption of food-production the biological processes, formerly operating in small isolated groups, were ' slowed down '; for the consequent rapid expansion of population broke down the segregation requisite.

The doctrine of evolution has raised human history above the domain of miraculous revelation or romantic fiction. It has imposed on historians the rigorous methods of natural science, but at the same time offered fresh interpretative concepts and heuristic devices. It has not provided a new extraneous agency to replace discredited deities or fates, nor revealed a short cut to conclusions that should obviate the collection of facts.

---

[3] A New Theory of Human Evolution, 1948.

[4] Leslie A. White. ' Diffusion *v.* Evolution: an Anti-evolutionist Fallacy ' *American Anthropologist*, vol. 47, 1945, pp. 339–356.

ANTIQUITY, XXXI, 1957

# Evolution and Diffusion

by Leslie A. White

*University of Michigan, Ann Arbor*

HOW are cultural similarities in non-contiguous regions to be explained? This has been an important question in ethnology for many years and still remains so to this day. E. B. Tylor termed it the 'great problem, the solution of which will alone bring the study of civilization into its full development as a science'.[1] It has been important for two reasons: first because it involves a theory of, or attitude towards, culture in general; culture is thought of as having one kind of nature if these similarities are explained in one way, but having a different nature if explained in another. Secondly, the question is important because we wish to know in the case of particular instances whether similarities are due to independent or to a common origin; quite apart from general theory we wish to know, and feel that it is important to know, whether, for example, the practice of mummification in aboriginal Peru originated indigenously or whether it had its origin, as some have believed, in ancient Egypt.

Lewis H. Morgan faced this question squarely almost a century ago when he attempted to explain similarities among kinship systems in widely separated regions. And he gave what is probably the most refined answer to it that we have ever had. Similarities may be due, he said, to (1) borrowing from one another; (2) accidental invention by different peoples in disconnected areas; (3) spontaneous growth or development in similar conditions of society; or to (4) inheritance through biological descent from a common original source.[2]

Descent from a common ancestry was probably the favourite explanation of non-adjacent cultural similarities among prescientific observers or students of culture. Thus real or imagined likenesses led to claims that this or that American Indian tribe was descended from the Greeks, the Phoenicians, the Welsh, or one of the Lost Tribes of Israel. Morgan believed that he could prove the Asiatic origin of the American Indian by demonstrating that a type of kinship system was found on both continents. And he used this argument to explain the distribution of clan organization rather than resorting to independent development or diffusion. Tylor once called the theory of common descent one of 'the two possible answers' to the question of cultural similarities,[3] although he was perfectly alive to the possibility of diffusion of culture without migration of people.

Most anthropologists since the days of Morgan and Tylor have, however, concerned themselves with but two of Morgan's possibilities, namely, independent development or diffusion. As R. H. Lowie has succinctly put it: explanation of 'cultural resemblances . . . between peoples of diverse stock . . . commonly narrows to a choice between two alternatives. Either they are due to like causes . . . or they are the result of borrowing'.[4]

---

[1] 'How the problems of American Anthropology present themselves to the English mind', *Science*, vol. IV, 1884, p. 547.

[2] Morgan, 'A Conjectural Solution of the Origin of the Classificatory System of Relationship', *Proceedings of the American Academy of Arts and Sciences*, vol. VII, 1865–8, p. 461. Morgan later considered each of these possibilities with regard to kinship systems at some length in *Systems of Consanguinity and Affinity of the Human Family* (1871), pp. 500–8.

[3] *Loc. cit.*

[4] *Primitive Society* (1920), p. 7.

It is significant to note that the premises and objectives of two of the three major schools of ethnological theory—the evolutionist and the diffusionist, or historical—are congruent with these two alternatives.

The assumptions upon which the evolutionists based their theory of independent origin and development were simple: man was a common and constant factor the world over and throughout history; his needs were uniform and the means of satisfying them were much the same everywhere. Therefore the results of man's behaviour, the cultures of mankind, were alike in their fundamental features. There was a ' natural logic of the human mind ', argued Morgan, that brought about similarities of cultures on ' different and even disconnected continents; . . . deviations from uniformity in particular instances [were] produced by special [environmental] causes '.[5]

Daniel Garrison Brinton, unquestionably the most eminent American anthropologist during the 1890's, was an extreme independent-developmentalist. The discovery of the psychic unity of mankind was a ' little less than marvellous ', he maintained: ' its full recognition is the corner stone of true anthropology '. As a consequence of this uniformity of the human mind we find a ' parallelism of his [man's] development everywhere and in all time; nay, more, the nigh absolute uniformity of his thoughts and actions, his aims and methods, when in the same degree of development, no matter where he is. . . . Scarcely anything but his geographical environment . . . seems to modify the monotonous sameness of his creations '. The same motifs in mythology ' arose independently in Western Asia, in Mexico, and in South America, as well as in many intervening places, alike even in details, and yet neither borrowed one from the other, nor yet drawn from a common source '.[6]

In Germany, the theory of psychic unity and independent development was well established under the leadership of Adolph Bastian, with his concept of ' elementary ideas '.

In England, Tylor, who set the tone for much of anthropological theory, was much interested, as we have already noted, in the problem of cultural similarities in non-contiguous regions, and he was as willing to admit diffusion as independent development to account for them. For him, issues of this sort were not to be settled in *a priori* fashion by appeal to an abstract principle, but by a careful weighing of evidence in each case. The philosophy of cultural evolution which he expounded so effectively had, however, the effect of inclining one toward the theory of independent development.

Thus, during the latter part of the 19th century, in those countries where anthropology flourished as a science, the prevailing tendency was to account for resemblances between institutions, technologies, and beliefs in different parts of the world by arguing—or merely taking it for granted—that they were natural responses of a uniform mentality.

But during the closing years of the last century a reaction against evolutionism and its corollary, independent development, set in both in the United States and in Germany. Opposition to the theory of cultural evolution arose from various sources, reactionary and irrational as well as scientific. But opposition to the independent developmentalists came from those who could demonstrate in a convincing manner the diffusion of certain traits, and from the exposure of extravagances of some of the exponents of psychic unity, such as Brinton, who went so far as to maintain that the similarities between the mythology of the Iroquois and their Algonquin neighbours was due to psychic unity rather than to borrowing.

---

[5] *Ancient Society* (1877), p. 18; see also pp. vi–vii.

[6] ' The Aims of Anthropology ', Presidential address before American Association for the Advancement of Science, *Proceedings*, 1896, pp. 4, 10.

Thus there arose diffusionist, or historical, schools of ethnology in both Germany and the United States. Friedrich Ratzel was the pioneer in Germany. He was followed by Fritz Graebner, Leo Frobenius, Bernhard Ankermann, and others; and after the decline of Graebner, leadership of this school was assumed by Father Wilhelm Schmidt and Father Wilhelm Koppers. In the United States, Franz Boas led the attack upon evolutionism and espoused the historical interpretation of culture. In England, W. H. R. Rivers announced his ' conversion ' to diffusionism in his presidential address to Section H, ' The Ethnological Analysis of Culture ', in 1911; this was followed by his *tour de force* of historical reconstruction in *The History of Melanesian Society* (1914). And shortly thereafter the diffusionist theories of G. Elliot Smith and W. J. Perry came into prominence.

Obviously, the theory of man-and-culture held by the diffusionists differed from that of the evolutionists. To the latter, the peoples of the world were not only alike psychologically, they were creative as well. Natural, spontaneous responses to environmental, or ecological, situations not only made for cultural uniformities; they produced a multiplicity of inventions as well: any people could invent fire, canoes, agriculture, bronze, clans, totemism, or writing; and any people *would* invent these things given the proper environmental setting and the requisite degree of cultural development. To the diffusionists, on the other hand, man was by nature uninventive. Novelty was rare and only a peculiarly favourable concatenation of factors could produce new cultural features. When they were produced, however, they could spread easily from one people or region to another.

Naturally, there were differences in the tenacity with which the rival theories of independent development and diffusion were held by various ethnologists, and in the extent to which they were carried as explanatory devices. There were wise and balanced moderates like Tylor who carefully weighed each situation and evaluated it in terms of its own factual matrix. Morgan, too, although an exponent of psychic unity, frequently recognized diffusion as an important and universal process of culture change. On the other hand, there were extremists like Brinton who all but denied borrowing altogether, or like Elliot Smith who went to the opposite extreme and virtually denied invention to all but the ancient Egyptians.

Some of the diffusionist opponents of evolution found themselves obliged to explain the growth of culture in terms of borrowing. Thus we find R. H. Lowie asserting that ' cultures develop mainly through borrowings. . . .'[7] This view is valid in so far as it recognizes the prevalence and importance of diffusion, but it ignores the fact that metallurgy and the alphabet, for example, cannot diffuse until they have been developed.

Emphasis upon diffusion has led many an anthropologist to equate, or to identify, anthropology with history. ' Whatever else the investigator of civilization may do ', says Lowie, ' he *must* be an historian.'[8] A. L. Kroeber has called historians ' anthropologists whose work is made easy for them by the possession of written and dated records '.[9] And more than one anthropologist has accepted F. W. Maitland's dictum that ' by and by anthropology will have the choice between being history and being nothing '.[10] It is obvious, of course, that much of anthropology is indeed history; but it is equally plain that it is also much more.

If the evolutionists sometimes went to unwarranted extremes and committed errors, so have the diffusionists. The extravagances of Elliot Smith are too well known to require

[7] *Op. cit.*, p. 441.
[8] *Ibid.*, p. 4.
[9] *Anthropology* (1923), p. 192.
[10] Paul Radin, for example, has used it as an epigraph for *The Method and Theory of Ethnology* New York, 1933).

critical comment here. And limitations of space do not permit even a review of critiques of the Graebner-Schmidt-Koppers group. The ' American Historical School ', as Goldenweiser called it, has been criticized by some of its own members as well as by others. Of the latter, Radcliffe-Brown has frequently inveighed against ' hypothetical ', or ' conjectural ', history, but, unfortunately, he identified evolution with history[11] and consequently rejects the former along with the latter. Edward Sapir, whose *Time Perspective in Aboriginal American Culture* (1916) was hailed by Father Schmidt as ' one of the best attempts ' by American ethnologists to develop a method of historical interpretation, came eventually to declare that ' the historical explanations now [1927] in vogue [are] often exceedingly dubious at best. . . .' [12] And Leslie Spier, who in 1921 undertook an historical reconstruction of the Plains Indian sun dance, remarked years later that ' some of the recent attempts at historical reconstruction . . . impress a moderately conservative individual as being highly tenuous or even downright careless and frivolous. . . .'[13]

We have no wish to minimize or belittle the achievements of the American culture historians. Their reconstruction of the culture history of the New World as a whole is necessary as a scientific hypothesis regardless of the fate that may befall it in the future. And there have been many instances in which valid reconstructions have been made on a small scale. But there has been far too much of ' conjectural history . . . invented *ad hoc* in order to account for actual and observable fact, in which therefore the known and empirical is " explained " by the imaginary and unknowable ' (Malinowski).[14] As examples of this we might cite the following: ' That Acoma got its clan system from the Hopi is proved [*sic*] by the fact that the paternal aunt gives her godchild one of her clan's stock of personal names, a naming practice peculiar to the Hopi ' (E. C. Parsons).[15] Or the following amazing statement by Franz Boas: ' The physical appearance of the Bella Coola proves [*sic*] that at one time they must have intermarried to a great extent with the Bella Bella. Through these marriages the peculiar customs of the Coast tribes were first introduced among them.'[16]

Many exponents of diffusionism have not been content to demonstrate the prevalence and importance of the spread of culture from one people or region to another ; they have gone farther and have maintained that the process of diffusion negates all theories of cultural development. Thus Lowie has declared that ' the extensive occurrence of diffusion . . . by itself lays the axe to the root of any theory of historical laws '[17] (i.e. cultural evolution). It is significant to note, too, that the historical schools of both Boas and Father Schmidt were intensely anti-evolutionist in attitude.[18] But, as we have argued in ' Diffusion vs. Evolution: an Anti-Evolutionist Fallacy ' (*American Anthropologist*,

---

[11] See the present writer's distinction between these concepts in *The Science of Culture* (1949), pp. 7ff., and ' History, Evolutionism and Functionalism: Three Types of Interpretation of Culture ', *Southwestern Journal of Anthropology*, vol. 1, pp. 221–48, 1945.

[12] 'Anthropology and Psychology ', in *The Social Sciences and Their Interrelations*, Wm. F. Ogburn and Alexander Goldenweiser, eds. (1927), p. 104.

[13] ' Problems arising from the cultural position of the Havasupai ', *American Anthropologist*, vol. 31, 1929, p. 218.

[14] ' Social Anthropology ', *Encyclopaedia Britannica*, 14th ed., 1929, vol. 20, p. 863.

[15] ' Early Relations between Hopi and Keres ', *American Anthropologist*, vol. 38, 1936, p. 559.

[16] *The Mythology of the Bella Coola Indians: Memoirs of the American Museum of Natural History*, vol. 2, 1898, p. 123.

[17] *Op. cit.*, p. 434.

[18] *Vide* Leslie White, ' Evolutionism in Cultural Anthropology: a Rejoinder ', *American Anthropologist*, vol. 49 (1947).

vol. 47, 1945), there is no conflict between the occurrence of diffusion and evolutionist theory. What the Boasians did here was to confuse the culture history of a people with the evolution of culture as such. They argued, for example, that because some African Negro tribes obtained iron-working by diffusion it was not necessary for ' all peoples to go through the same stages of cultural development ' and that therefore the theories of the evolutionists were fallacious. But the evolutionists never claimed that ' every people had to go through the same stages of cultural development '; they merely argued that metallurgy or clan organization or writing was something that had to be evolved, or developed, from earlier stages or forms. To the evolutionist it made no difference whether a given people obtained a trait by diffusion or developed it indigenously; it was the evolution of the culture that they were concerned with, not the cultural experiences of this or that tribe. There is thus no incompatibility between diffusion and evolution of culture. Both are fundamental and universal processes of culture change. And, far from conflicting with each other, they actually work together, the one producing new elements or configurations, the other spreading them to other cultural systems.

We come now to the question: What conclusions have anthropologists reached with regard to the independent development or the diffusion of specific cultural elements after decades of investigation? It would not be easy, by any means, to answer this question. The diffusion of certain traits such as the use of tobacco or the alphabet has been demonstrated, and the spread of other traits can be reasonably inferred. On the other hand, the family, in one form or another, undoubtedly arose spontaneously everywhere as a natural response to primate nature, with variations being produced by particular local conditions. The variety of techniques for making fire on low cultural levels suggests multiple and independent origins.

With regard to such major problems as the relationship of cultural development in the New World with that in the Old, which has been a major anthropological problem for more than a century, one must, we feel, conclude that the question is still an open one. In the mid-1920's, the prevailing view among American anthropologists was that culture had developed in the New World independently of the Old. And this view was, if anything, strengthened rather than weakened by the Pan-Egyptian theory of Elliot Smith. But in more recent years, especially since Robert Heine-Geldern and Gordon Eckholm have marshalled evidence and argument in support of Asiatic influence,[19] one is not nearly so sure.


## Summary

The cultures of the world, both prehistoric and modern, exhibit both similarities and differences in various parts of the earth. To account for these similarities and differences has been one of the major tasks of cultural anthropology. It has been of fundamental importance because it has required a general theory of man-and-culture as well as the determination of a host of particular facts. Some scholars have leaned toward the theory of independent development, others toward diffusion. Extravagances and errors have been committed in both camps. And it has even been thought by some that the lion of diffusion would eventually devour the lamb of evolution, but this has been shown to be an unfortunate error. It is now clear that both of these processes of culture change are universal and fundamental; each one merits as much respect as the other. And we may be sure that whatever fads and fashions may come and go in anthropology, the problems of evolution and diffusion will remain with us for a long time to come.


[19] ' Significant Parallels in the Symbolic Arts of Southern Asia and Middle America ', *Selected Papers of the XXIXth International Congress of Americanists*, Sol Tax, ed., vol. I, 1951.

ANTIQUITY, XXXI, 1957

# Worms and Weathering

by R. J. C. Atkinson

IT is a little surprising that in spite of the relatively high standard of excavation techniques current in Britain today so little attention has been paid by practical excavators to the processes of formation of the sites which they dig.

All of us recognize, of course, that a site consists of a sequence of deposits, some formed deliberately and usually rapidly by man, and others more slowly by nature; and that some processes of formation, such as erosion and filling by the plough, are still continuing today. But there seems to be a widespread assumption (though it is difficult to be sure of this, since such things are seldom discussed) that once a constituent layer of a site has been formed, and sealed by another layer above it, it becomes immediately fossilized and remains unchanged until examined by the excavator, perhaps several millennia later. It is equally widely supposed that once an object, large or small, has come to rest in or upon a deposit it will remain in that position for all time. In short, the tacit assumption is that once formed the nexus of finds and deposits which constitutes a site is almost wholly static, and that change, if it occurs at all, occurs only at the surface.

Happily this assumption is not universal, as is shown by the observations of a few excavators specially interested in soil problems. Professor O'Kelly, for instance, has recently shown[1] how undisturbed fossil soils (or what most of us call ' old turf lines ') may undergo chemical changes after burial which radically alter their colour, and may lead (indeed, have led) to their identification as artificial ' ritual ' floors.

The purpose of this article is to give additional evidence that the assumption of a static state in archaeological sites is false, and even dangerous. Most of the phenomena described are not new: some of them were first described over a century ago, and others have been recorded, though not recognized, in recent excavation reports. Nor can I claim to write with any kind of scientific authority. My only aim is to draw the attention of my fellow-excavators to some facts largely ignored hitherto, though they may seriously affect the interpretation of excavated sites. It seems to me that if these phenomena are not more widely recognized and more critically studied, we are all, as excavators, attempting to interpret the end-products of processes which we do not understand.

## 1. The Archaeological Significance of Earthworms

The earliest observations of the part played by worms in the formation of surface soil were published by Charles Darwin in 1837[2]. He continued these studies throughout his life, and summarized his conclusions in his book *The Formation of Vegetable Mould through the Action of Worms with Observations on their Habits*, published in 1881 and since reprinted several times[3]. This fascinating work is largely unknown to archaeologists,

[1] *Journ. Cork Hist. and Arch. Soc.*, LVI (1951), pp. 29–44.

[2] *Trans. Geological Soc.*, V (1837), p. 505.

[3] The most recent edition was published by Faber in 1945 under the title *Humus and the Earthworm*.

though it includes many specific references to ancient sites, and a whole chapter (ch. IV) entitled ' The part which worms have played in the burial of ancient buildings '. Since Darwin's death surprisingly little new information has been assembled, and there are still many gaps in our knowledge of the life-history, distribution and effects of the various species of worm. The chief recent publications, on which much of this section is based, are listed in the bibliography on p. 233.

The total of British species of earthworm is about twenty-five, but only a quarter of these occur at all commonly. The largest, *Lumbricus terrestris*, about a foot long in the adult state, is known as the Common Earthworm, though perhaps wrongly, since though widespread it is dominant only locally. A better candidate would be a smaller, lighter-coloured species, *Allolobophora caliginosa* (3–4 in. long), which constitutes a high proportion of all local populations.

Worms of all species live in burrows in the soil, formed partly by the insertion and expansion of the pointed front segments of the body in interstices of the soil, and partly by actual ingestion of the earth. Some of the soil ejected at the hinder end after digestion is used to line the walls of the burrow, particularly where it passes through coarse or angular material; the remainder is voided either in empty spaces below ground level, or on the surface in the form of the familiar worm-casts.

In his book Darwin made no distinction of species, and thus by implication attributed to worms in general behaviour which is now known to differ according to the species. It now appears that only two closely related species, *Allolobophora nocturna* and *Allolobophora longa*, habitually eject casts on the surface, while all the other species void their ejecta below ground. Again, these same two species, together with the Common Earthworm, not only feed on soil ingested underground, but also eat leaves and other vegetable matter lying on the surface, which they draw into the mouths of their burrows. Such forays usually take place at night and towards dawn; and since worms exposed on the surface are rapidly paralysed by the ultra-violet component of daylight, it is literally true that the early bird catches the worm whose nocturnal foraging has been overtaken by the morning light.

Most of the British species live only in the top 6–8 in. of soil, but the three surface-feeding species habitually burrow to much greater depths where the conditions allow. Most excavators will remember having encountered worms occasionally at depths of 6 ft. or more, particularly in dry or cold weather. Indeed, it is clear that worms in general are very sensitive to changes in temperature and humidity, and at least five species react to extremes of drought and cold by retreating to greater depths than usual, and awaiting improved conditions curled up in a ball in a spherical chamber formed at the bottom of a burrow. Darwin observed that these chambers are often lined or floored with small stones a few millimetres in diameter, which may act as insulators against the loss of heat and moisture to the surrounding soil. The two surface-casting species appear regularly to aestivate in this way, irrespective of the prevailing temperature and rainfall; but the Common Earthworm seems to be active throughout the year, though it retreats to lower depths in dry weather.

The absolute size and the component species of local worm populations is markedly influenced by the type of soil inhabited, and also by its previous agricultural history. The highest numbers are found in old pastures, undisturbed by cultivation, of alkaline or neutral reaction. Here one of the casting species (*A. nocturna*) is often dominant, and may form half the total population; though as it seems to have a slow rate of reproduction, its dominance may not be established until a decade or more after the ground has been put down to grass. The breaking-up of such pastures (which are confined chiefly to the Low-

# PLATE I

*a*. SECTION THROUGH THE STONEHENGE BANK; THE TOP LAYER OF WEATHERED NATURAL CHALK HAS BEEN REMOVED AND CAN BE SEEN AT THE BASE OF THE SECTION

*b*. FILLING OF STONE-HOLE 31 AT STONEHENGE, SHOWING WEATHERED AREA AGAINST THE FACE OF THE STONE

PLATE II

RECTANGULAR PATTERN OF ROMAN CENTURIATED FIELD-SYSTEM SHOWING UP BENEATH MODERN CULTIVATION ABOUT 40 MILES SOUTH-EAST OF TUNIS

*Photo*, Institut Géographique National

# ANTIQUITY

## A Quarterly Review of Archaeology

*Edited by O. G. S. Crawford C.B.E., Litt.D., F.B.A.*

VOLUME XXXI

1957

*Printed by*
THE WHITEFRIARS PRESS LTD.
LONDON AND TONBRIDGE

# Contents, Volume XXXI


PAGE

Editorial Notes – – – – – – – – 1, 57, 121, 185

*Articles:*

Mycenaean records: a review. By T. B. L. Webster – – – 4

Salina: a prehistoric village in the Aeolian Islands. By Madeleine Cavalier – – – – – – – – – – 9

The Shrine of St Peter: a review. By Erik Sjöqvist – – – 15

Excavations at Jamestown, Virginia. By John L. Cotter – – 19

Four Spanish hill-forts. By A. H. A. Hogg – – – – – 25

The First Universities. By Humphrey Humphreys – – – 33

Graffiti of Ships at Tarxien, Malta. By Diana Woolner – – – 60

Swiss Pile-dwellings. By E. Vogt – – – – – 68

Jericho and its Setting in Near Eastern History. By Robert J. Braidwood – – – – – – – – – – 73

Reply to Professor Braidwood. By Kathleen M. Kenyon – – 82

The Brummagen Philhellene. By A. J. B. Wace – – – – 85

The Campignian Tradition and European Flint-mining. By W. Creighton Gabel – – – – – – – – 90

Wild Doings in the Theological College. By A. R. Burn – – – 93

Notes on Minoan Linear A. By Cyrus H. Gordon – – – 124

The Culture Sequence of Bactria. By F. R. Allchin – – – 131

Braves, Beakers and Battle-axes. By H. W. M. Hodges – – – 142

The Longstone, Mottistone. By Jacquetta Hawkes – – – 147

Old and New Rock Engravings in Natal, South Africa: a Zulu Game. By B. D. Malan – – – – – – – – – 153

The Skye House. By James Walton – – – – – – 155

Evolution and Man. By Ronald Singer – – – – – 188

CONTENTS

Tools Makyth Man. By KENNETH OAKLEY – – – – – 199
The Evolution of Society. By V. GORDON CHILDE – – – – 210
Evolution and Diffusion. By LESLIE A. WHITE – – – – 214
Worms and Weathering. By R. J. C. ATKINSON – – – – 219
Civil Service Archaeology: a review. By SIR MORTIMER WHEELER 234
Akkadian Tablets in Minoan Dress. By CYRUS H. GORDON – – 237
Some Recent Books and Articles – – – – – 96, 163, 241
Books Received – – – – – – – – – – 164
Notes and News – – – – – – – – 36, 97, 167
Reviews of Books (*see list following Index*) – – 42, 105, 173, 242

# PLATE III

RECTANGULAR PATTERN OF ROMAN CENTURIATED FIELD-SYSTEM SHOWING UP BENEATH MODERN CULTIVATION ABOUT 40 MILES SOUTH-EAST OF TUNIS

*Photo,* Institut Géographique National

PLATE IV

RECTANGULAR PATTERN OF ROMAN CENTURIATED FIELD-SYSTEM NEAR MAHDIA, TUNISIA

*Photo,* Institut Géographique National

land Zone of Britain) leads to a reduction of the worm population, and in particular of the casting species, which may fall after a few years by as much as 75 per cent.

In the Highland Zone, where unimproved rough pasture on acid soils is far more common, the worm populations are generally smaller and contain a narrower range of species, from which the casting and deep-burrowing types are often largely or wholly absent. Here, in contrast to the south, cultivation may actually increase the local population, through the improvement of the soil structure and the reduction of acidity by lime and fertilizers. Since many of the archaeological effects of worms are attributable to the casting and deep-burrowing species alone, it is clear that the significance of worms for the archaeologist is much greater in the Lowland Zone.

The absolute size of worm populations should come as no surprise to excavators, among whom even the most tender-hearted and squeamish soon tire of throwing on to the dump innumerable specimens, often already bisected by pick or trowel. Populations of half a million to the acre, or more than a hundred to the square yard, are by no means uncommon, and figures six times as great have been recorded for favourable habitats. The weight of worms below the surface of a field may thus equal the weight of livestock that it will support.

The amount of fine soil transported from lower levels to the surface, in the form of worm casts, is much larger than is commonly supposed. Amounts ranging from 2 to 24 tons per acre per annum have been recorded in Britain, and up to 36 tons on the Continent. This represents the formation of a surface-layer of fine mould varying in thickness (in Britain) from $\frac{1}{50}$ to $\frac{1}{4}$ in. per annum, or from $\frac{1}{5}$ to $2\frac{1}{2}$ in. in ten years. Practically all this material is brought to the surface in the autumn and spring, since the casting species are dormant during the summer, and tend also to reduce their activity and retreat to lower levels during the colder part of the winter. The non-casting species also contribute substantially to the mixing of the surface layer of soil, though their effects are less immediately visible, since their ejecta are voided below the surface.

The principal archaeological effects of worms are considered under various headings below.

(*a*) *The burial of small objects*

Most farmers and gardeners know well that material deposited on the surface of a field or lawn, even if not soluble by rain, will gradually disappear and after a lapse of several years will be found, still as an integral layer, at some distance below the surface. The most obvious example is the mass of stones which litter the surface of any ploughed field, yet soon disappear when the field is put down to grass.

The sinking of such material is wholly due to the activities of worms. On the one hand, soil is brought up to the surface as worm-casts and gradually accumulates there, while on the other hand disused burrows below the surface are constantly collapsing, and thus producing local subsidences of the overlying soil. The net effect of these two related processes is to cause objects lying on the surface to sink below it, while the absolute level of the surface remains unchanged. Flints, potsherds, coins and the rest of the common currency of the excavator are naturally affected in this way no less than other material.

The rate of burial is variable and depends on a number of factors. It is related primarily, of course, to the size of the local worm population, and in particular to the proportion in that population of the two casting species. And even if the size and composition of the population is assumed to be constant, the rate of burial will not be uniform except in the top 6–8 in. of soil, in which the majority of worms normally live. Below this depth objects will sink with increasing slowness, since the active proportion of worms,

and hence the subsidence of empty burrows, decreases in proportion to depth. There seems no reason to doubt, however, that objects will continue gradually to sink in this way, if the required conditions exist, to the maximum depth to which worms normally penetrate, which is about 6 ft.; though below about 8 in. from the surface the rate of sinking per annum will fall off progressively, and may soon reach only a tenth or even a hundredth of the rate observed in the topmost layer of soil.

Observations by Darwin and others suggest that on uncultivated ground in the Lowland Zone the initial rate of vertical displacement for small objects is about 2 in. in ten years, and that where the necessary depth of soil exists this rate, or even higher rates, may be maintained for up to 30 years, during which the total displacement may amount to as much as 12 in.[4] But it must be realized that even where the rate of burial is rapid, the total vertical movement which is possible is limited by the available depth of soil in which worms will work. While they will burrow in material which contains a high proportion of stones or rubble, they will not normally penetrate pure rubble, gravel or other coarse and angular material in the absence of admixed earth; and still less the natural subsoil. Thus if the soil layer is only 9 in. in depth, objects dropped on the surface can be displaced downwards only by that distance, or slightly less, a process which may on the average take 40 or 50 years; thereafter all vertical movement necessarily ceases. Moreover, even where greater depths of soil exist, the displacement of objects may at any stage be brought suddenly to a halt, if the existing surface is covered by a fresh deposit (such as a bank or mound) sufficiently thick to cause an upward movement of the worm population. In this way the activity of worms in the former surface soil, now buried, will cease, and no further vertical displacement will occur of the objects contained in it.

The inescapable conclusion to be drawn from these facts is that in many cases *significant archaeological finds have been displaced downwards from the position in which they were originally deposited; and in some cases at least the amount of displacement may have been sufficient materially to alter the apparent stratigraphic relationships of the objects concerned.*

Two examples will be enough to show how the burying activities of worms may affect the interpretation of stratified finds. In many Romano-British and mediaeval excavations, for instance, floors are uncovered consisting of opus signinum (cement), tessarae or tiles. The surface of these floors is often sealed by a layer of fine soil containing sherds, coins and other débris of occupation, which is itself covered by a thicker layer of rubble derived from the decay and collapse of the surrounding walls. Such ' occupation layers ' are usually referred to a reoccupation of the building by squatters, after it has been abandoned by the original inhabitants and has perhaps already become partially ruined.

The exact processes by which such ' occupation layers ' have been formed is seldom if ever discussed in excavation reports (indeed, one wonders if it is not supposed that it found its way into the building attached to the squatters' boots). In fact, such ' occupation earth ' is almost certainly the product of earthworms, which penetrate cracks in apparently solid floors and gradually build up a layer of castings on their surface. Naturally, under these conditions this surface layer of soil will accumulate more slowly than on an open field, and it may take a century or more to build up a layer four inches thick. Meanwhile

[4] It may be objected, of course, that on an occupied site the constant activity of men and animals may inhibit the activity of worms, which are known to be timid and to retreat downwards when the surface is vibrated by the passage of feet. But it has been observed at Stonehenge, the most frequented of ancient monuments, that the scatter of chalk crumbs which represent the sites of Colonel Hawley's dumps has already sunk $2\frac{1}{2}$ in. in the last 30 years. It must be remembered that much of the activity of worms takes place at night, when even the most frequented site is undisturbed.

this layer will itself have been colonized by worms, whose activity will bury coins and other objects dropped on it; and some of these will be displaced downwards so that they eventually come to rest immediately on the surface of the original floor.

Now finds discovered in this latter position will usually be interpreted as belonging either to the very latest stage of the original occupation, or to the initial stages of ' squatting '; in either case they will be held to date approximately the period at which the building was initially abandoned. But in fact, as a result of the processes outlined above, the objects found *on* the floor may be of widely differing dates, and will include not only those dropped just after the abandonment of the building, but also those of much later date which have been displaced from above. Consequently, if the normal practice is adopted of dating a horizon by the *latest* objects contained in it, the date assigned to the initial abandonment of the building may be significantly later than it should be.

Admittedly, the amount of such errors may often be insignificant, since at least near the wall of a room the ' occupation layer ' will usually be sealed fairly rapidly by the accumulation of rubble above it; but it is by no means impossible that towards the centre of a large room, with low walls of substantial build, the covering of the ' occupation earth ' by rubble may be delayed by a century or more.

Another common case in which the activity of worms may lead to serious misinterpretation of the evidence occurs in the silting of pits and ditches. Such silting usually exhibits four basic divisions. On the bottom, and up to half the depth of the ditch, there is coarse or rapid silting, formed chiefly from the weathering of the sides (FIG. 1, A). Above this is a thin deposit of earthy streaks, due to the undermining and collapse of the surface soil on the lips of the ditch (FIG. 1, B). Both these stages are normally completed within a few years at the most. Thereafter, silt forms much more slowly (FIG. 1, C), and usually contains a far higher proportion of earth, particularly in structures of the 2nd millennium B.C., such as barrow-ditches, which collect considerable quantities of wind-blown surface-soil[5]. Finally, at the top, there is the ' turf ' layer of virtually stone-free soil which supports the surface vegetation (FIG. 1, D).

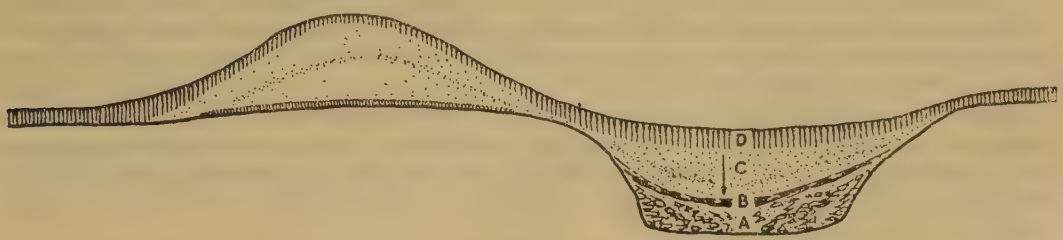



FIG. 1. SECTION OF TYPICAL BANK AND DITCH, SHOWING DISTRIBUTION OF TURF AND SEQUENCE OF SILTING.

Since the first two stages of silting take place rapidly, the formation of the lowest levels of the third stage may be regarded as virtually contemporary with the original excavation; and in the absence of finds from the primary silt (those in the secondary silt, being derived possibly from the adjacent surface-soil, are unreliable), objects occurring at the base of the tertiary silt may often be used to date the original excavation. But it is clear that such finds *may* have been displaced vertically through considerable depths of silt (FIG. 1, arrow), particularly where a high proportion of windblown or other soil

[5] *P.P.S.*, XIX (1953), pp. 138–46.

encourages the activity of worms. Since such vertical displacements may take centuries to achieve, the date ascribed to the structure on such evidence may be grossly in error.

The classic case of the probability of an error of this kind is afforded by the Y and Z Holes at Stonehenge[6], in which the main part of the silt consists of earth, apparently wind-blown and almost free from stones or rubble[7]. This silt contained Iron Age and Romano-British sherds at quite low depths, which have in the past been held to date the holes to not earlier than the last few centuries B.C.[8] But once the possibility of vertical displacement is recognized, it becomes clear that this pottery must have been deposited *on the surface of the silting* many centuries after the digging of the holes; and in fact other evidence suggests a date for the holes as much as a thousand years *earlier* than that of the pottery they contain[9].

These examples, and particularly the last, may perhaps be regarded as extreme cases; and certainly it would be foolish to deny that in many instances the vertical movement of objects, though it has undoubtedly taken place, has no significant effect on their interpretation. None the less, one may justly say that the excavator who ignores the capacity of worms to displace small objects downwards, either through ignorance or wilfully, does so at his peril.

*(b) The subsidence of structures*

It is clear from the observations of Darwin and others that the same processes which lead to the burial of small objects apply also to much larger ones, though the amount of displacement is naturally smaller. Darwin showed, for instance, that some of the fallen stones of Stonehenge (probably stones 12 and 127), which at the time of their fall must have come to rest on the surface, are now buried to depths of 9½ and 4 in., resting on, or very close to, the underlying chalk; and that this subsidence may be attributed wholly to the removal by worms of the surface soil on which they lay. The *apparent* depth of burial is even greater, since the worm-casts accumulate round the edges of the stones to produce a local elevation of the surface[10].

Darwin also noticed, as many modern excavators can confirm, that worms are capable of penetrating the apparently solid surviving walls and floors of ancient buildings, and that the interstices of the masonry are commonly filled with fine dark soil ejected by them. Some undermining must thus certainly have taken place, with a corresponding subsidence of the entire building; and where the site is built on accumulated hill-wash (as are the Roman villas at Chedworth and Northleigh, for instance), the amount of subsidence may have been substantial. Though the matter is never referred to in excavation reports, it is after all necessary to explain why the floor levels of such buildings are invariably found *below* the present ground surface.

More obvious subsidence is frequently visible where floors sag over earlier pits or ditches. This is usually attributed merely to the gradual consolidation and shrinkage of

---

[6] Atkinson, *Stonehenge* (1956), pp. 20–21.

[7] *P.P.S.*, XIX (1953), pp. 138–40.

[8] *Antiquity*, III (1929), pp. 80–81; Cunnington, *Stonehenge and Its Date* (1935), p. 17; Grimes (ed.), *Aspects of Archaeology* (1951), pp. 283, 292.

[9] Atkinson, *Stonehenge* (1956), pp. 71–6.

[10] It may be objected that returfing and the accumulation of rubbish may account for the apparent local rise of the surface at Stonehenge, and I have heard this rise estimated at a foot or more. In fact measurements show that the additional thickness of topsoil within the stones, including that due to returfing, amounts to only 2½ in. Both the fallen stones examined by Darwin lie *outside* the sarsen circle.

the underlying filling; but in fact it is probably due chiefly to the transport by worms, through small cracks in the floor, of earthy material from below. Even where no earlier structures exist below the floor, there may be subsidence to a depth of 6 in. or more, if the floor was laid directly on the original surface soil. The amount of sinking is not uniform, being greatest at the centre and least towards the edges, where the activity of worms has been halted sooner by the accumulation of rubble from the collapsing walls. Darwin observed and published two instances of subsidence of this kind, in the Roman villas at Abinger and Brading. It will be realized that such subsidence is necessarily counterbalanced by the formation of a layer of fine soil on the surface of the floor, and it is this which constitutes the ' occupation layer ' commonly encountered in this position.

### (*c*) ' *Split peas* '

Every excavator is familiar with the thin and irregular deposits of pea-sized pebbles, free from admixed soil, which are commonly found on the bottom of pits, post-holes and shallow ditches, on the buried surface of floors and large stones, and on the line of junction of a layer of earthy filling with one of coarser material below it. Though this phenomenon is sufficiently commonplace to have acquired the colloquial names of ' split peas ' or ' dried peas ', it has apparently never been explained. There can be little doubt, however, that these pebble-layers arise from the long accumulation of the small stones used by worms to line the terminal chambers of their burrows. Since these chambers are made and occupied when the worms are forced to retreat downwards to avoid extremes of drought and cold, the pebbles tend naturally to be concentrated at the lowest levels to which worms can penetrate locally.

A related phenomenon is the thin skin of fine powdery earth which commonly lines the sides of pits and post-holes, especially where the filling of the hole consists of coarse or very compact material. This earthy skin seems to consist of the fine soil used by worms to line their burrows, which follow preferentially the sides of the pit, where slight shrinkage of the filling permits penetration. There is a danger that this phenomenon may be misinterpreted as the remains of some decayed lining of organic material, such as basketry, if the formative action of worms is ignored.

In some cases the presence of this fine skin of soil, and of empty burrows in it, encourages the growth of roots, which may form a close matted network dividing the filling of the pit from the subsoil in which it is dug.

### (*d*) ' *Old turf lines* '

The occurrence of fossil soils, colloquially called ' old turf lines ', beneath barrows and banks is known to every excavator. Where the overlying deposits are substantial, the fossil soil is normally intact and sharply marked, though possibly somewhat compressed. But where the bank or mound was originally only a foot or less in height, or has been reduced to this thickness by cultivation or erosion, the buried soil is usually diffuse and merges gradually into the rubble of the overlying mound; or indeed it may be absent altogether. At Stonehenge, for instance, where the maximum thickness of the bank of the main earthwork is now only just over 12 in., the old turf line consists of a diffuse streak of mixed soil and chalk rubble, about 4 in. thick (PLATE I, B) (FIGS. 2 and 3), without any sharp division between it and the overlying rubble of the bank proper. On the Avenue

and Cursus [11], where the height of the banks is even less, no trace of the old turf line remains.

The usual explanation for the absence of an old turf line is that the topsoil was deliberately stripped from the site of the bank before it was built. But this is a curious and unnecessary thing to do; and though it might be justified on ' ritual ' grounds on sites such as the Avenue and Cursus, there is little place for a ' ritual ' of this kind in the case of purely domestic earthworks[12]. Moreover, it is significant that beneath larger earthworks, even if of ' ritual ' character, the old turf line is invariably intact.

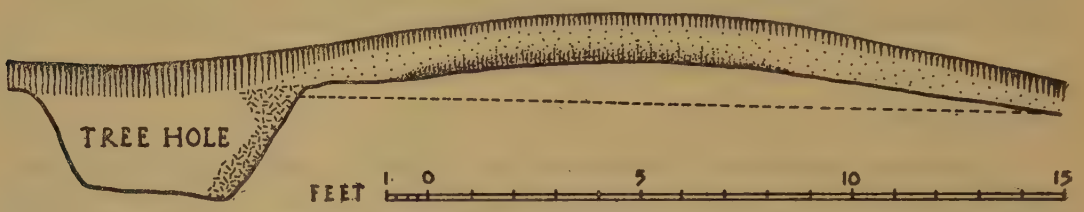



FIG. 2. SECTION OF THE STONEHENGE BANK, EXCAVATED 1954.
The broken line continues the slope of the adjacent undisturbed chalk surface.

In fact, the partial or complete disappearance of the buried soil from beneath low banks is almost certainly due to the activity of worms, which bring up the soil *through* the overlying bank and deposit it on the surface, provided that the thickness and density of the bank is not so great as to discourage penetration. The limiting thickness appears to be about 12 in. for ' pure ' chalk rubble, but will be greater if the material of the bank contains even a small amount of admixed earth.

It is clear that we are dealing here with a potential source of serious errors in interpretation. If excavation of a shallow bank reveals no buried soil, and also exposes datable objects beneath the bank, it will usually be assumed that the surface soil was stripped off deliberately before the bank was built; and the date of the bank will be equated with that of the objects found beneath it, since their deposition appears to be bracketed between two phases of construction (stripping and bank-building). But if it is realized that a buried soil can subsequently disappear through the action of worms, it follows that the objects may have been contained in that soil, and may thus antedate the construction of the bank by many centuries.

On most chalk sites there is a simple means of determining whether the absence of an old turf line is due to worms, or to the deliberate stripping of the soil. On such sites the undisturbed surface soil normally contains at its base a layer of frost-fractured natural flints, which cannot be moved by worms. If this layer is present at the base of a bank, it shows that the fossil soil must formerly have been sealed by the bank, even if it is no longer present; if there are no flints beneath the bank, they must have been removed by deliberate stripping of the ground before the bank was built[13].

---

[11] *Arch. Journ.*, CIV (1947), pp. 12–16. The excavator, the late Dr J. F. S. Stone, subsequently agreed that the absence of an old turf line was due to the activity of worms, and that the ' consolidated chalk dust ' or ' compo ' layer at the base of the bank was in fact the weathered but undisturbed surface of the natural chalk.

[12] e.g. the Late Bronze Age enclosures on Plumpton Plain (*P.P.S.*, I (1935), pp. 25, 29, figs. 8, 9, 12), New Barn Down (*Sussex Arch. Coll.*, LXXV (1934), p. 149, fig. 4) and Thorny Down (*W.A.M.*, XLVII (1937), p. 646, pl. III).

[13] *Arch. Journ.*, CVIII (1951), p. 8.

Even where the old turf line is buried beneath a substantial bank, and has thus been undisturbed by worms since its burial, the former activity of worms in it may give a clue to the previous history of the site. During the recent excavations at Wandlebury (Cambs.), for instance, it was observed that the outer rampart of the fort covered an old turf line about 6 in. thick. The upper part of this buried soil consisted of fine stoneless earth, while the lower part contained an admixture of chalk crumbs and flints characteristic of soils disturbed by agriculture. It seems likely, therefore, that the ground on which the rampart stood had been under plough not much more than a decade before its construction.

(*e*) *The erosion and silting of earthworks*

There seems no doubt that worms have played, and continue to play, a substantial part in the erosion of mounds and banks and the silting of their ditches. Wherever worm-casts are brought to the surface of a slope, however slight, they will tend to be washed *downhill* by rain, while in dry weather they crack and break up into small pellets. which roll downwards under the action of gravity and the wind.

The rate of transport downhill by rainwashing appears to be very small, chiefly because the cast material is soon flattened, and adheres to the surface soil between the leaves and stems of the grass. Some measurements by Darwin suggest that the flow of soil off a slope of 10 degrees is sufficient to add only some 6 *sq. in. per century* to the cross-section of the surface soil at the foot of the slope, or on the surface of the adjacent ditch. The amount added in the form of dry pellets of soil is probably substantially greater, but more difficult to calculate.

Darwin also suggested (ch. VI) that even on flat sites the transport of comminuted worm-casts by the wind may affect the rate of surface-silting in ditches. Measurements which he took in the ditch of a disc-barrow near Stonehenge showed differences in the thickness of the ' turf ' which were apparently attributable to the transport of soil by the wind in the prevailing S.W.–N.E. direction.

It is fairly certain that worm-casting is one of the main factors affecting the differential distribution of ' turf ' on slopes and in silted depressions (FIG. 1), which is observable even on ground undisturbed by man. At Stonehenge, for instance, the average thickness of surface soil on flat ground is 10½ in., whereas on adjacent slopes of only 1½ degrees or less it is 7½ in. The difference is usually much more marked on earthworks, where the turf on slopes is substantially thinner than on undisturbed ground, while at their base and in the ditch it is substantially thicker (FIG. 1). It is this differential distribution which gives rise to the characteristic soil-mark of a ploughed-out earthwork, the thicker soil in the ditch and at the inner base of the bank showing as dark bands, contrasted with the lighter marks of the eroded bank and the outer lip of the ditch.

There is also reason for supposing that the whole of the surface soil on a slope may, like a glacier, be moving imperceptibly downhill. As Darwin suggested (ch. VI), the burrows of worms on a slope will tend always to close up towards the downhill side. Though the amount of soil movement in each individual case is infinitesimal, the cumulative effects over many years may be considerable. Some evidence to confirm this apparently unlikely hypothesis came from a careful examination in 1954 of the topsoil in two sections of the Stonehenge ditch. Besides sherds and numerous fragments of the stones, this was found to contain thousands of pieces of natural frost-weathered patinated flint. Since these cannot have grown *in situ* (and can readily be distinguished from the flint gravel deliberately deposited elsewhere on the site), they must have been transported laterally, by some natural process, from the slopes of the adjacent bank and counterscarp bank. This lateral movement raises grave doubts of the reliability of any conclusions

based on the stratigraphical position of finds in the upper part of the ditch-filling, since any individual object may have been subjected to the same process, and may thus have been dropped on the bank centuries before it found its way into the ditch. The rate at which this flow of surface soil may take place is unknown, and experiments are being initiated to gain further information.

(*f*) *The mechanical properties of soils*

Since the soil ingested by worms and excreted on or near the surface consists always of the finer particles only, it follows that the prolonged activity of worms in an undisturbed soil should lead to a grading of particle sizes throughout the soil profile, the mean particle size increasing progressively with depth. The existence of such grading has in fact been established by mechanical analysis.

It is also known that the constant passage of soil through the alimentary tracts of worms will in time alter its physical composition, the coarser particles being reduced in size by physical attrition and chemical attack. The net effect of worm activity is thus to increase the clay and silt fractions in a soil at the expense of the fine and coarse sand fractions.

These effects are of some importance, now that the analysis of soils is becoming a recognized adjunct of archaeological research. It is easy to see, for instance, that the analysis of a fossil soil beneath an earthwork, in which no worms have been present for many centuries, may differ significantly from that of the adjacent soil outside the earthwork, which has been inhabited continuously by worms; though originally both soils were identical in composition. Due weight must thus be given to these processes in interpreting differences in mechanical composition, or in the distribution of particle sizes in soil profiles.

## II: The Weathering of Natural Subsoils

It is of course a commonplace of geographical studies that the materials composing the surface of the earth's crust are continuously subjected to weathering, by which they are broken up and their soluble constituents dissolved away, the insoluble residue accumulating on the surface as particles which, combined with organic matter, constitute the soil.

The processes of weathering are partly mechanical and partly chemical. Mechanical weathering consists of the breaking up of the surface of the subsoil, chiefly by frost and to a lesser extent by the penetration of plant roots. This has the effect of vastly increasing the surface which is vulnerable to chemical attack. In chemical weathering the rock is attacked by percolating surface-water, which is in effect a weak acid, so that some of its basic constituents (chiefly calcium and magnesium carbonates and felspars) are dissolved. The acidity of the surface-water is derived in part from the atmosphere, in the form of carbon dioxide dissolved in rain-water[14], and in part from complex humic acids formed in the soil itself from the decay of organic matter. Carbon dioxide is also exhaled by the roots of plants.

It may be noted in passing that these processes of weathering are undoubtedly accelerated by the activity of earthworms, which not only increase the permeability of the soil and thus facilitate penetration of surface-water to the subsoil, but also contribute to the production of humic acids by the incorporation of vegetable matter.

[14] During the last century rain-water has contained an increasing proportion of sulphuric acid, formed from sulphur compounds emitted by the burning of coal, chiefly for industrial purposes. Today the annual rainfall of Britain contains about 6 million tons of sulphuric acid, or about three times the annual production of the acid by the chemical industry.

Hitherto little evidence seems to have been collected for the rate at which various natural rocks and sediments are being weathered, or have been weathered in the (geologically speaking) recent past. It is generally assumed, however, at least by field archaeologists (if indeed they think about it at all), that the rate of weathering is exceedingly slow; so slow, in fact, that no detectable change can have taken place in the last four thousand years, so that the level and conformation of the surface of the natural subsoil in excavated sites can safely be assumed to be the same as when the site was first built or occupied.

The purpose of this section is to suggest that this assumption is a false one, and that in many cases the level of the natural subsoil has been lowered by weathering and denudation, since the initial occupation of a site, by amounts which if ignored may lead to serious misinterpretation of the excavated structures.

The evidence for these degrees of weathering, which may amount to 20 in. in the last 4000 years, comes from observed differences in level between the subsoil beneath a bank or mound, where it has been protected from weathering, and that outside the earthwork, where it has not been protected.

The first instance of such a difference came to my notice during the excavation of two sections of the Stonehenge bank in 1954 (FIGS. 2 and 3 and PLATE I, A). Here we found that beneath the bank, and to a lesser extent beneath the counterscarp bank, the level of the underlying chalk gradually rose towards the centre, the maximum difference of level being about 14 in. It seemed that this difference could be explained only by assuming that, since the time when the bank was built, the level of the whole of the surrounding downland had been lowered, through weathering, by at least the amount observed and probably more.

The convexity of the surface of the chalk buried beneath the bank shows that even after passing through the bank the percolating surface-water retained sufficient acidity to effect *some* weathering; and that the degree of this weathering was related to the thickness of the bank, being greatest at the edges where the thickness was least. This suggests that even beneath the centre of the bank some slight degree of weathering has taken place. Consequently the *absolute* depth of weathering since the bank was built almost certainly exceeds the observed difference of 14 in. Indeed, this is confirmed by recorded differences of up to 20 in. on sites of comparable age (and on the same chalk subsoil) where the depth of the bank has been sufficient to provide complete protection of the buried subsoil.

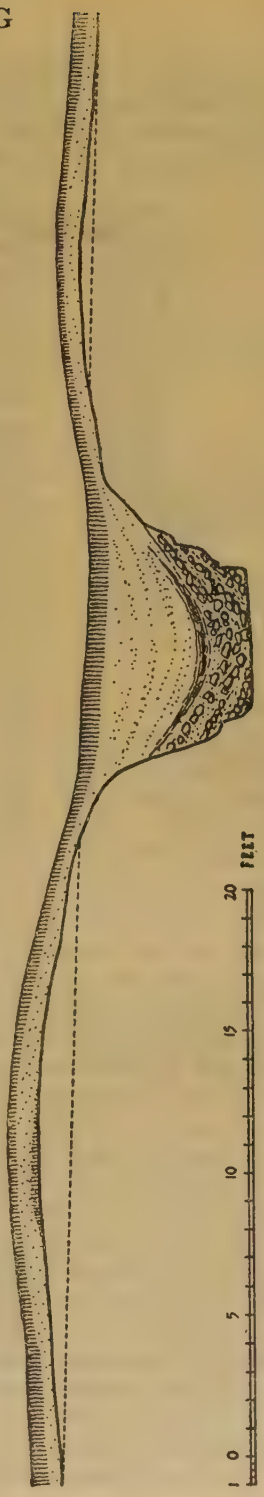



FIG. 3. SECTION OF THE STONEHENGE BANK, DITCH, AND COUNTERSCARP BANK, EXCAVATED 1954.
The broken line continues the slope of the adjacent undisturbed chalk surface.

Confirmatory evidence of the weathering of chalk by the acidity of surface-water was found at Stonehenge in 1956, in another form. Sections dug radially up to the inner faces of the bluestones at the entrance (nos. 31 and 49) showed that at their bases the compact chalk rubble packing had been dissolved by water trickling down the stones, leaving a wedge-shaped space into which the overlying soil and flints had sunk (FIG. 4) (there is a thick artificial deposit of flint gravel beneath the turf in this area). This effect (PLATE I, B) is due, of course, to the fact that the stones, facing directly the prevailing wind and rain, act as catchment areas so that the concentration of rain around the bases on this side is greater than elsewhere.

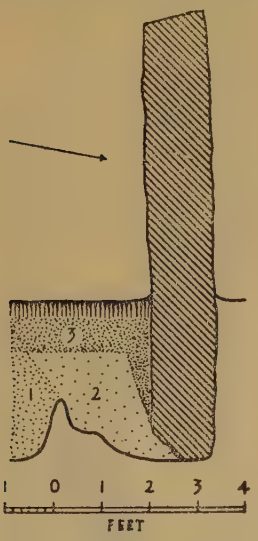



Fig. 4. RADIAL SECTION THROUGH BLUE STONE 49 AT STONEHENGE.

1. Stone hole, period II. 2. Stone hole, period IIIc. 3. Topsoil and flints. The arrow shows the direction of the prevailing winds and rain.

The observation of these phenomena at Stonehenge naturally led me to search for comparable evidence in published reports of excavations. The results of a limited search, confined mainly to publications in my own library, are given in the table on p. 232. Though in no way exhaustive, they show that these phenomena are widespread, and are not confined merely to the softer rocks and sediments.

It should be noted, however, that the evidence from published reports is not as informative as it might be. Many published sections are either wholly diagrammatic, or are printed at scales too reduced to permit accurate measurement. In many cases, too, the section does not extend far enough on to undisturbed and unprotected ground to allow a reliable estimate of the depth of weathering. Moreover, such estimates can be made only where the undisturbed ground is either flat or on a uniform slope; many earthworks are sited on the crest of convex slopes, and the convexity of the underlying natural rock, though it may be due in part to weathering, may also be accounted for, in unknown degree, by the convexity of the original slope.

But in spite of these restrictions, it is clear that there is acceptable and widespread evidence for the lowering of the level of the natural subsoil on many sites by significant amounts, since their original construction. The rate of weathering is obviously variable, and it seems impossible to derive any ' standard rates ' from the figures quoted. All such weathering will, of course, leave an insoluble residue which becomes incorporated in the surface soil. But in most cases this residue will amount to no more than 5 per cent, or less, of the volume weathered. The weathering of 20 in. of chalk will thus add only 1 in., at the most, to the overlying soil, so that the net lowering of the surface will be 19 in.

One might expect that the highest rates of weathering would obtain during periods of maximum rainfall, and some slight evidence for this can be found. At the Iron Age hill-fort of Poundbury (Dorset), for instance, there is a difference of about 2 in. between the levels of natural chalk protected successively by the earlier and later portions of the inner rampart [15]. These two constructions can hardly be separated by more than three centuries, giving a minimum rate of weathering of $\frac{2}{3}$ in. per century. Again, at Maiden Castle, at least 4 in. of weathering seems to have taken place in only a century and a half; that is, between the building of phases 1–2 and 3–4 of the western inner rampart [16]. These rates are significantly greater than the highest over-all rates observed elsewhere on chalk (the

[15] *Ant. Journ.*, XX (1940), pl. LXXI.
[16] Wheeler, *Maiden Castle* (1943), pl. IX.

maximum is about ½ in. per century), and it may be noticed that the weathering in question took place at a period during which the rainfall is generally supposed to have been heavier than the modern average.

It may be objected, of course (and particularly by those whose motto is *omne ignotum mullius momenti*), that much or all of the observed differences in level of the natural subsoil may be due to the effects of ancient or recent agriculture. While this may be true of earthworks standing on a slope (where a negative lynchet may be formed along the lower edge), it can hardly apply on flat ground, and still less on sites like Stonehenge which one may safely assume never to have been under plough. Moreover, it is doubtful whether the ploughing of the surface increases at all the rate of weathering of the underlying rock. Indeed, the reverse is probably the case, since on cultivated ground the percolation of surface-water to the subsoil, on which weathering ultimately depends, is inhibited by the greater evaporation-surface presented by the loosened soil.

The archaeological significance of these unexpectedly large depths of weathering is clear. On a site like Stonehenge, where the unprotected chalk has been weathered in the last 4000 years to a depth of more than 14 in., the great majority of pits, stone-holes and post-holes must originally have been a foot or more deeper than they are today. This immediately simplifies the task of reconstructing the methods used to erect the stones. It also explains the present fantastic shallowness of many of the stone-holes at Avebury, which one may now suppose originally to have been nearly twice as deep as they are at present[17]; and it probably accounts in large part for the fallen position of so many stones in this and other free-standing stone circles.

It is clear, too, that where pits or post-holes have been dug to original depths of a foot or less below the surface of the subsoil, the depth of subsequent weathering may have been sufficient to have obliterated them entirely. This may well explain the almost total absence of timber houses of the 2nd millennium B.C. in the Lowland Zone of Britain, in spite of abundant evidence of population in other forms (e.g., barrows). It is surely significant that the few timber houses which we can date earlier than the late Bronze Age are all in the Highland Zone on rocks less vulnerable to weathering; and that where stake- or post-structures of the 2nd millennium B.C. do occur in the Lowland Zone they are always protected from weathering by the mounds of barrows.[18]

I am well aware that some of the conclusions I have drawn are startling, and may even be said to cut at the roots of some accepted canons of archaeological interpretation. Their final proof or rejection must depend upon much wider and more critical observation and record of the phenomena discussed than is at present available, and it is mainly in the

---

[17] Many of the stone-holes are now only 18 in. deep in the chalk. Mr St George Gray's excavations (*Archaeologia*, LXXXIV (1934), pl. XL) suggest that the depth of weathering is also about 18 in.

[18] But it cannot be assumed that stakeholes will always be intact even under a barrow. In barrows on Thames gravel near Oxford (Stanton Harcourt, *Oxoniensia*, X (1945), pp. 21–32, fig. 8; Cassington, *ibid.*, XI/XII (1946–7), pp. 5–26, fig. 5), consisting of a soil core with a peripheral gravel capping, the level of the subsoil beneath the core was 6 in. or more *below* that beneath the gravel capping. The difference is presumably due to the neutralization of the acidity of percolating surface water by the calcareous constituents of this capping. It is thus clear that even though the soil cores of the mounds still stood to heights of between 2 and 3 ft., the natural gravel below them had been weathered away to a depth of at least 6 in., and probably more. Shallow pits or holes beneath the mound may thus have been obliterated entirely.

hope of stimulating such specific observations that this article has been written. Shots in the dark may not always be very accurate; but at least they serve to wake sleepers from their beds.

I am indebted to Dr Ian Cornwall for several valuable emendations to the text of this article, which he has been kind enough to read before publication; and to Professor J. D. Evans, Mr Bernard Wailes and Mr Nicholas Thomas for information about excavations conducted by them but not yet published in detail.

## TABLE

The sites are listed approximately in order of their apparent age.

| *Site* | *Subsoil* | *Depth of Weathering* | *Publication* |
|---|---|---|---|
| Long Barrow, Thickthorn, Dorset . | Chalk | 20″ | *P.P.S.*, II (1936), pp. 23–5 and fig. 9. |
| Long Barrow, Skendleby, Lincs. . | Chalk | 12″+ | *Archaeologia*, LXXXV (1935), p. 63, fig. 10. |
| Chambered Barrow, Notgrove, Glos. | Limestone | 6″–8″ | *Archaeologia*, LXXXVI (1936), pl. XLIII. |
| Earthwork, Lyles Hill, Co. Antrim . | Basalt | 12″ | Evans, *Lyles Hill* (1953), fig. 6. |
| Bank, Stonehenge, Wiltshire . . | Chalk | 14″ | Personal observation. *Supra*, figs. 2 and 3. |
| Cursus, Stonehenge, Wiltshire . | Chalk | 13″ | *Arch. Journ.*, CIV (1947), p. 13, fig. 3. |
| Avenue, Stonehenge, Wiltshire . | Chalk | 12″ | Personal observation. |
| Henge Monument bank, Cairnpapple, West Lothian. | Basalt | 6″ | *P.S.A.S.*, LXXXII (1948), pp. 81–3, fig. 6. |
| Barrows 1–3, 8, 9, Crichel Down, Dorset. | Chalk | 12″–16″ | *Archaeologia*, XC (1944), pp. 63–72, figs. 10, 12, 13, 17, 19. |
| Bell, bowl and disc barrow, Earls Farm Down, Amesbury, Wilts. | Chalk | 4″–8″ | Unpublished; information from excavator. |
| Bell barrow (III), Snail Down, Everleigh, Wilts. | Chalk | 18″ | Unpublished; information from excavator. |
| Barrow 6A, Lansdown, Somerset . | Oolitic limestone | 12″ | *Ant. Journ.*, XXX (1950), pp. 44–46, fig. 6. |
| Barrow Sutton 268′, Llandow, Glamorgan. | Lias limestone | 8″ | *Archaeologia*, LXXXIX (1943), pp. 89–126, pl. XLV. |
| Barrows 3, 5, 6, Chewton Mendip, Somerset. | Lias limestone | 12″ | *Proc. Somerset A.N.H.S.*, XCIII (1947), pp. 43–57, fig. 9. |
| Barrow II, Swarkeston, Derbyshire. | Flint gravel | 12″ | *Derbyshire A.J.*, LXXV (1955), pp. 123–9, fig. 1. |
| Barrow, Lockington, Leics. . . | Gravel | 12″ | *Trans. Leics. A.H.S.*, XXXI (1955), pp. 17–29, fig. 2. |
| Barrow 7, Radley, Berks. . . | Thames gravel | 5″–8″ | *Oxoniensia*, XVII/XVIII (1952–3), pp. 26–31, fig. 14. |
| Barrows Sheeplays 293′ and 279′, Llantwit Major, Glamorgan. | Lias clay | 6″–9″ | *Ant. Journ.*, XXI (1941), pp. 97–118, pl. XXIX. |

| *Site* | *Subsoil* | *Depth of Weathering* | *Publication* |
|---|---|---|---|
| Simondston Cairn, Coity, Glamorgan. | Lias clay | 6″ | *Archaeologia*, LXXXVII (1937), pp. 129–66, pls. I, LXV. |
| Pond Cairn, Coity, Glamorgan . | Lias clay | 10″ | |
| Barrow, Talbenny, Pembrokeshire . | Boulder clay | 7″–11″ | *Arch. Journ.*, XCIX (1942), pp. 1–32, fig. 5. |
| Barrow I, Barnby Howes, Yorks. . | Boulder clay | 6″ | *Yorks. A.J.*, XXXIX (1956), pp. 9–31, fig. 4. |
| L.B.A. enclosure, Plumpton Plain, Sussex. | Chalk | 10″+ | *P.P.S.*, I (1935), pp. 23–5, fig. 9. |
| L.B.A. enclosure, Thorny Down, Wilts. | Chalk | 6″ | *W.A.M.*, XLVII (1937), pp. 640–60, pl. III. |
| I.A. Settlement, West Harling, Norfolk. | Glacial gravel | 6″ | *P.P.S.*, XIX (1953), pp. 1–40, fig. 4. |
| Hill fort, Caesar's Camp, Wimbledon, Surrey. | Sand | 9″ | *Arch. Journ.*, CII (1945), p. 17, fig. 2. |
| Hill fort, Bredon Hill, Glos. . . | Gravel on limestone | 7″–14″ | *Arch. Journ.*, XCV (1938), pl. XXXVII. |
| Hill fort, High Rocks, Tunbridge Wells, Sussex. | Sandstone | 12″ | *Sussex A.C.*, LXXXII (1941), p. 106, fig. 2. |
| Hill fort, Bonchester Hill, Roxburghshire. | Boulder clay | 4″–9″ | *P.S.A.S.*, LXXXIV (1950), pp. 113–36, fig. 5. |
| Hill fort, Stanwick, Yorks. . . | Boulder clay | 6″ | Wheeler, *Stanwick Fortifications* (1954), pl. VII. |
| Roman road, Bokerly, Hants. . | Chalk | 12″ | Pitt-Rivers, *Excav. in Cranborne Chase*, III (1892), pl. CLXIII. |
| Roman road, Birtley, Northumberland. | Sand | 8″ | *Arch. Ael.*, 4 ser., XVII (1940), p. 60, fig. 3, sect. V. |
| Roman road, Edlingham, Northumberland. | Clay | 7″–9″ | *Arch. Ael.*, 4 ser., XVII (1940), pp. 69–73, fig. 3. |
| Medieval (?) linear earthworks, Beaulieu and Stoney Cross, Hants. | Gravel | 2″–5″ | *Proc. Hants. F.C.*, XVI (1947), pp. 166–7, figs. 1, 2. |

## BIBLIOGRAPHY

STEPHENSON, J. *The Oligochaeta.* (Oxford, 1930.)

EVANS, A. C. and GUILD, W. J. McL. ' Studies on the relationships between earthworms and soil fertility ', *Annals of Applied Biology*, XXXIV (1947), pp. 307–30; *ibid.*, XXXV (1948), pp. 1–13, 181–92, 471–84, 485–93.

ROOTS, B. I. ' Famous Animals—7: The Earthworm ', *Science News*, XXI (Penguin, 1956), pp. 102–17.

# Civil Service Archaeology: A Review *

by Sir Mortimer Wheeler

THIS book, published in February 1957 though dated 1956, is ' the first in the series of Ministry of Works Archaeological Reports ', and as such is a landmark. True, it is a landmark in a desert of frustrate expectation; for since the beginning of the last war the Ministry has fathered several hundreds of archaeological excavations, of which some have been (more or less) adequately published through unofficial media, others ' await ' publication, and some are never likely to be published at all. In other words, the problem has exceeded the capacity or opportunity of the Department concerned, and ANTIQUITY has not always been silent in the matter. ANTIQUITY is not always right in its censure; but it is more often right than wrong, and of few independent critical reviews in any branch of human activity can that be said. Anyway, on the present occasion at least we may put out all flags in welcome to the newcomer.

But first let us get this little matter of anti-Ministry criticism into perspective. In the attempt I beg forgiveness for the personal approach of one who has been the familiar of four successive Chief Inspectors of Ancient Monuments and has fought with all of them without (I hope) foregoing the privilege of lunching happily with them afterwards. Sir Charles Peers I first met in 1913 and last saw shortly before his death. From his full and expressive mind I learned more than I can say; and after 1924, when I found his untended workmen gaily and irresponsibly heaving skeletons, potsherds and coins out of the Roman towers of Caerwent, he may on occasion have learned a little from me. Anyway, our intermittent conflicts, in which no punches were pulled, merely tightened our friendship. His successor, Bushe-Fox, a man of shrewd judgment if restricted knowledge, was my first master in the craft of digging; to State archaeology below the ground he brought some part of that understanding which Peers had lavished on upstanding structures. Bryan O'Neil, who followed him, was in a sense my pupil, but of his Chief Inspectorship the last war prevented me from seeing much. His tenure was one of unparalleled difficulty, and I should like to think that, for all the defensive obstinacy and seeming unco-operativeness with which he was liable to counter criticism, his achievement—often behind the scenes, for he was essentially a solitary—in the development of his Department (and, incidentally, in planning the official publications now in question) is remembered with a proper gratitude. Where he fell short was, not in intent, but in the enlistment of effective support and interest beyond the limits of a staff no longer commensurate with its growing problem. Of the present Chief Inspector it would be unfitting for me to say more than that twenty-seven years ago we worked closely as colleagues, and the happy memory remains.

From the Department's controlling personnel I turn to its achievement in conservation. In doing so, it would be ungracious not to recall the sterling contribution of two generations of exceedingly capable Ministry architects, of whom A. Heasman and J. A. Wright were best known to me. To a branch of the Ministry's staff for which there is a far rosier future in the building of post-offices than in the removal of ivy from Tintern Abbey a

* Excavations at Jarlshof, Shetland, by J. R. C. Hamilton. Edinburgh: H.M. Stationery Office, £3 3*s.* 0*d.*

special measure of appreciation is due for the devoted skill with which a whole technique of conservation has been accumulated and is still under constant review. In this matter the measure of advance over the years can be understood by comparing the primitive beginnings at Old Sarum with the relative perfection of Rievaulx Abbey or Richborough Fort. It has been my fate to see archaeological conservation in a good many lands and to practise it in two, and I am second to none in my admiration, for example, of the sixty trained craftsmen of the Monuments Historiques who are restoring the eastern arm of the shattered cathedral of Nevers to something like its pre-war grandeur. But I have no hesitation in proclaiming this: that, whatever I and others may say in the untrammelled privacy of our Ancient Monuments Boards, or even in the columns of ANTIQUITY, our Ministry of Works is unsurpassed if not unmatched in its all-round technical prowess and its sympathetic understanding of its task as conservator and protector.

Then finally in our leisurely approach to Mr Hamilton's volume we come to this contentious matter of excavation. Before we throw stones, let us glance at the size and shape of the problem. The unprecedented agricultural and constructional activities of the war and post-war period have involved the destruction of countless archaeological sites redeemable, if at all, only by extravagant compensation. In the circumstances, it is very greatly to the credit of the Ministry that it has in fact attempted, by more or less summary excavation, to save something from the burning. Some of this work has been well done, some of it badly or inadequately. In the latter cases the Ministry is inevitably blamed, nor am I prepared to affirm that it has always displayed a proper sense of responsibility. There has, I am afraid, too often been a clear deficiency of administrative alertness, and a continuing lack of that liaison with useful local interests at which I have already hinted. At the same time, by and large the Department is certainly far less inept than selected bad-cases might make it appear to be. That its activities need scaling upwards there can be no doubt; it may be that on its staff it carries more passengers than its relatively small numbers can afford, and unquestionably the talent available for its multitudinous excavations is and will remain insufficient. But let us remember, with something more than charity, to recall in our criticism the overriding quality to which I gave expression at the end of the last paragraph.

The highly important excavation now recorded in Mr Hamilton's pioneer volume falls outside the rescue work just referred to. It represents the skilled uncovering of a complex site during a long process of intelligent conservation, unembarrassed by extraneous urgencies. This is our Ancient Monuments Department at its best. The site, Jarlshof on the southern tip of Shetland, has in a manner been known to fame since Sir Walter Scott, in *The Pirate*, described the 16th century 'Lairds' Hoose' or 'Yarlshof' of which fragments still crown the mound, and gave it a fictitious occupant. But in 1897 the veritable owner, John Bruce, began a series of excavations which were continued intermittently under varying direction until 1952, and the whole site has now been laid out as a show-place by the Ministry on lines indicated in an appendix to the present report.

The story of the site, as now revealed, covers between 2000 and 3000 years, beginning with a nebulous 'Stone Age' scatter (whatever that may imply chronologically in this remote latitude) and developing into a substantial Bronze Age settlement marked by oval huts with central hearths, lateral cubicles and inner cattle-byres. The crofters were tillers and fishers as well as herdsmen, and seem to represent a continuing native population to which migrant bronze-smiths—from Ireland, it is suggested, but perhaps east-coast infiltration is as likely—brought the art of casting socketed axes, swords, gouges and sunflower-pins in clay moulds. On the heels of the bronze-smiths, pottery more or less reminiscent of east-coast Iron Age A came along, and subterranean larders were introduced.

But whether these innovations represent immigration or culture-drift it would be rash to say; the author prefers the former.

Less doubt attends the next phase. Sand swept across the site, and it was on the new dunes that a typical broch or circular peel-tower was built, presumably in the typical broch period, the 1st or 2nd century A.D. Here some measure of colonization from Orkney is postulated, though the change may in fact have been little more than that of the laird. Not long afterwards, circular houses with radial piers—the familiar ' wheel-houses ', evolved from local Bronze-Age prototypes—were added alongside, again with underground store-houses; and these buildings or their lineal descendants were still in occupation when, about A.D. 800, occurred the invasion which was to give Jarlshof its special distinction in northern archaeology.

I remember how, in the middle 'thirties, the discovery of Viking houses at Jarlshof by Dr A. O. Curle brought something nearly approaching a flush of excitement to the honoured countenance of that seasoned and unshakable Scot. The emotion was sternly checked, but some of us lesser folk were less inhibited, not least those who had been properly nourished on the sagas and had striven with the ghostly Glam in the darkness of Thorhall's house. Here at last was the real thing: not perhaps very much to look at, but a good plain longhouse, with sides bowed a little as by builders whose longships were never far from their minds. No more of your squalid stone wigwams, infested not by human beings but by culture-creeps. Why, here is the Viking himself, scribed for us on a stone; wide-eyed and regal, his bearded and curly head set upon a sturdy neck. And there is his ship, sketched summarily on another stone, with its crew, mast and steering-oar (though I cannot follow Mr Hamilton when he sees in this vaguely generalized graffito a close and significant resemblance to the Kvalsund boat, with an inferential dating to the 7th–8th centuries). And if we go into his longhouse through one of the opposed doorways—precursors of the medieval screens-passage—we find on one side his small kitchen, raised on a low platform, and on the other the main hall with timber aisles and a dais along each wall. In Scandinavia and Iceland, with affinities even in the Low Countries, the type is already sufficiently known, but it has been left to Mr Hamilton to illuminate the whole subject by analysing the traditional Shetland croft both structurally and etymologically and thus showing that in both aspects it is the living descendant of the Viking longhouse. This section of his book (pp. 102–106, with diagram) is an outstanding contribution to our knowledge of the early Norse homestead.

The book is, as it should be, first and foremost a factual statement, and as such will endure. No extensive analysis of the finds is attempted, though their treatment generally reaches adequacy and the illustrations are excellent. I should have liked a more ample diagrammatic demonstration, in section, of the relationship of the middens (cardinal evidence) with the structures, but appreciate the special difficulties which must have confronted Mr Hamilton in dealing with a site on which so many others had already worked. One point only calls for comment of another kind. The admirable (coloured) over-all plan is placed *loose* in a pocket inside the cover. This is fatal, nothing less. No pocket plan survives library usage (unless in the most recondite libraries) for more than six months, and nothing is more infuriatingly baffling than an empty pocket. The plan, which is essential, should have been bound in on a flap long enough to enable it to be extended beyond page-width. For the rest, ungrudging gratitude is due alike to author and to publisher.

ANTIQUITY, XXXI, 1957

# Akkadian Tablets in Minoan Dress

by CYRUS H. GORDON
*Brandeis University, Waltham, Mass.*

1. In the September number of ANTIQUITY (no. 123, pp. 124–130), I suggested that the language of Linear A was Semitic. I leaned toward narrowing down the probabilities to West Semitic, but indicated the possibility of East Semitic (= Akkadian or Assyro-Babylonian) too. The clue to the identification of the language proved to be *ga-ba* MAN *62* (= Akkadian *gabba awîlū 62*) ' all the men = 62 ' (p. 128, n. 14aa). Subsequently I observed that the regular Mycenaean Greek totalling formula (e.g. *to-so* MAN *17* ' so many men: 17 ')[1] was as exact an equivalent of my Akkadian reading as one could expect in so different an idiom. From that moment on I approached Linear A with Akkadian in mind.

2. The new approach meant that Linear B *o*-signs would correspond to *u*-signs in the Akkadian syllabary, in which these two vowels are not distinguished; and that Linear B *e*-signs would often correspond to *i*-signs in the Akkadian syllabary, in which the two vowels are not regularly distinguished.

3. There is a sign that means ' received ' logographically because it frequently follows the personal name at the beginning of a tablet, and precedes the commodity issued to him.[2] The sign is *te* in Linear B. In accordance with the principle stated in §2, I modified it to *ti* for Linear A, to serve as the Sumerogram *TI* = *ilqī* ' he received '.

4. My predecessors had correctly deduced from contexts with tell-tale numerals, that three Linear A words meant something like (1) ' total (of the commodities already itemized)', (2) ' cumulative total ', and (3) ' owed ', respectively. With ' X ' and ' Y ' as unknowns, the words appeared as (1) *X-ro*, (2) *Y-to-X-ro*, and (3) *ki-ro*, in Linear B values. Nothing could seem more obvious than the assumption that *X-ro* meant ' total ' and *Y-to-* meant ' cumulative ' or ' grand- '. Yet this assumption led nowhere and, after months of frustration, I was obliged to abandon it and to regard *Y-to-X-ro* as a single Akkadian word suitable for indicating ' cumulative total '. The pattern of the word is such that it must have four consonants, of which the second is *t* and the last is *r* or *l* (for *r*-syllables cover *l* as well as *r*). In any language, such rigid requirements as to meaning and phonetic pattern, limit the possibilities quite narrowly. Moreover, there would be an external control over the word to be chosen, for the value assigned to *X* would have to yield sense for the word *X-ro* too. Akkadian *kitmuru* ' heaped up, accumulated '[3] fulfils the requirements. *Y-to-X-ro* thus becomes *ki-tu-mu-ru* in Linear A (see §2), and *X-ro* becomes *mu-ru*, which is the only way the scribes could write Akkadian *mullū*, ' paid out, issued '.

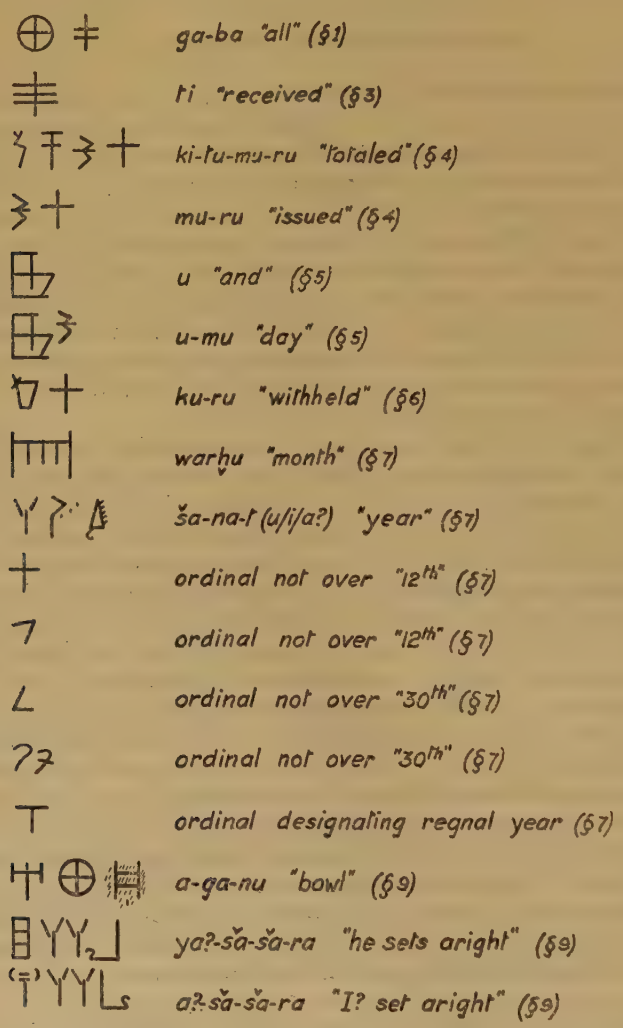
---

[1] All Mycenaean Greek words can be found, with full documentation, in the ' Vocabulary ' of Ventris and Chadwick, *Documents in Mycenaean Greek*. The passage I have quoted is from text 39 : 10, on p. 172.

[2] First noted by the late Professor J. L. Myres. I have learned this, and many other valuable clues, from the essay on Linear A, in Ventris and Chadwick, *op. cit.*, pp. 31–6.

[3] By coincidence, only the roots *gmr* ' complete ' and *kmr* ' heap ' supply suitable meanings and both of them would fall together in Minoan script; *ki-tu-mu-ru* could stand for *gitmuru* as well as *kitmuru*. I prefer *kmr* because two nouns derived from it (viz., *kimratu* and *kumurrū*) denote ' (arithmetical) total '.

5. To test the validity of the equations $X = mu^4$ and $Y = ki$, I re-examined every occurrence of both signs. $Y$ is among the rarest signs, so while other occurrences impose no obstacles, neither do they provide confirmation. Fortunately, however, $X$ is of high frequency. None of the occurrences of $X$ run counter to the reading *mu*, and one confirmed it; it follows the Linear B sign *o* (see § 2) to form the Akkadian word *u-mu* 'day' where




we expect the date formula in two tablets (HT 16 : 3 ; 20 : 5). This in turn showed that Linear B *o* is the Akkadian conjunction $u = \bar{u}$ 'and' in HT 15 : 4 and 103 : 3, to join either items (HT 103 : 3) or clauses (HT 15 : 4).

6. So far, so good. But what about the word for 'owed' that seemed to have the Linear B value of *ki-ro* (see §4)? Context shows that it corresponds functionally to Myce-

[4] This sign may prove to be *mo* of Linear B.

naean Greek *o-pe-ro* ' owed ' and I soon realized that it had to be a form of Akkadian *kl'* ' withhold ', whose D-conjugation provides the idiomatic antonym of *mullū*.[5] The Linear B *ro* is, of course, to be read *ru* (see §2), which may also be pronounced *lu* (cf. §4). Our equation *Y* = *ki* (cf. §4) paves the way for modifying Linear B *ki* to *ku* for Linear A, because *ku-ru* = *kullū*,[6] 'withheld ': the exact counterpart of *mu-ru* = *mullū* ' issued '.

7. In a date formula, the day (numbered not over 30th) must be accompanied by the month. In both tablets (see §5), the months are written logographically (' MONTH ') and followed by ordinals[7] (not over 12th; 13th, though conceivable in a leap year, is less likely).[8] The Old Babylonian order ' month-day-year ' appears in Linear A. HT 20 has only the month and day, but HT 16 adds the year thus: *ša-x-y* $n^{th}$. The only form of ' year ' that can fit this orthography (with three signs) is the status constructus *ša-na-t(u/i/a?)* = *šanat*. The HT 16 date formula is therefore to be read so:

*warah n* ' $n^{th}$ month ' (n = ordinal not over 12th)

*ūm n'* 'nth day ' (n = ordinal not over 30th)

*šanat n''* 'nth year ' (n = ordinal probably designating year of some king's reign).[9]

8. Cylinder seals found on Crete have long attested Mesopotamian influence there during the First Dynasty of Babylon (19th–16th centuries B.C.). An Akkadian dedicatory inscription of Naram-Sin, son of Ibiq-Adad, King of Eshnunna, was found on Cythera and published in 1853.[10] The note on this text by E. F. Weidner (*Journal of Hellenic Studies*, 59, 1939, pp. 137–8) is all the newcomer needs by way of basic bibliography and orientation. The fact now confronting us is that for two centuries before Greek (Linear B) was written, Akkadian in Minoan script was the *lingua franca* of the Aegean. How much earlier cuneiform Akkadian played that role there, the future will tell.

9. Although Mesopotamian penetration of the Aegean was essentially commercial, commerce does not tell the whole story. From a wide variety of sites have come quite a few religious objects bearing Linear A inscriptions. A bowl (Cn 7)[11] from Knossos bears a text that begins with *a-ga-nu* = *ag(g)ānu* ' bowl '. No less than five inscribed religious objects from different places, contain the word which according to Linear B values, would be read *ya-sa-sa-ra*, var. *a-sa-sa-ra*. It is hard to dissociate this from what appears in

---

[5] In the Nuzu tablets, the opposite of *umallī* (which can be apocopated to *umal*; see *Orientalia*, 7, 1938, p. 222, § 5.20) ' he has paid out ', is *ukal* ' he has withheld ' which has been mistaken for the D-conjugation of *kwl*. Nuzu Akkadian, while unclassical in many ways, is meticulous regarding the tenses. If Nuzu ' he has withheld ' were from *kwl*, the preterite tense would have been *ukīl*. The simplest way to explain Nuzu *ukal* is to regard it as the apocopated form of *ukallā* (from *kl'*) like *umal* (from *ml'*).

[6] This form explains *ku-lu-ù* on an Old Akkadian tablet (I. J. Gelb, *Old Akkadian Inscriptions*, Chicago, 1955, p. 265). Five names of people, present and accounted for, appear on the obverse; while on the reverse, after a large blank space, three more names are followed by *ku-lu-ù*. Here *ku-lu-ù* (= *kullu'ū* 'they have been withheld ') signifies that the three persons on the reverse have not been handed over.

[7] Elsewhere these logographic numbers seem to be fractions. In Akkadian the formation *parus* designates ordinals as well as fractions. Thus *šaluš* (with nominative case ending: *šalšu*) means ' (the) third (one) ' as well as ' (one) third '.

[8] Our Linear A evidence suggests Aegean affinities (via the Philistines) for the Judean calendar in which the months are numbered from 1st through 12th.

[9] Just as *UD*um 1kam is read *ūm* 1kam ' 1st day ' in Akkadian, *MU* 1kam ' 1st year ' should now be read *šanat* 1kam instead of *šattu* 1kam. I shall explain the formation on another occasion.

[10] Called to my attention by O. G. S. Crawford.

[11] Republished, with the HT texts, by G. Pugliese Carratelli, *Mounmenti Antichi*, XL (4*a*), 1945, col. 593–4.

classical Babylonian as *ušéšer* (perhaps (*y*)*ašayšar* in the Linear A dialect ?) ' I/he set(s) aright '.

10. That non-commercial texts are written in Akkadian, prompts us to seek the answer to our problem from the fact that the Aegean had a common culture but not a common native language. The hieroglyphic Cretan script enjoyed enough cultural prestige to triumph on its own soil over cuneiform. But if the linguistic situation was as complicated as the Odyssey (19 : 173 ff.) tells us of heroic Crete, it is easy to see why the international *lingua franca*, Akkadian, served a useful purpose on Crete; much as nowadays English does on the Indian Subcontinent, where there is a lot of common native culture, but no common native language.

## Addenda

*ku-ra* (HT 103 : 5) = *kullā* ' they (f. pl.) have been withheld ' (vs. m. pl. *ku-ru* = *kullū* § 6).

The formation of *šanat n* ' $n^{th}$ year ' (§ 7) is confirmed by Hebrew *šᵉnat haššᵉlīšīt* (etc.) ' $3^{rd}$ (etc.) year ' which occurs commonly in books written, or redacted, during and after the Babylonian Exile; thus several times in Kings (e.g., 2 Kings 17 : 6) and Jeremiah (e.g., 46 : 2); cf also Ezra 7 : 8; etc., etc.

# Some Recent Books and Articles

*The inclusion of a book in this list does not preclude its subsequent review*

ROMAN ROADS IN BRITAIN; vol. II, by IVAN D. MARGARY. Phoenix House, 1957, £2 10*s*. [The second and concluding instalment of Mr Margary's fine survey, covering Britain north of the Foss Way and the Bristol Channel, which of course includes the whole of Wales and Scotland. Vol. I was reviewed in ANTIQUITY XXX (No. 117; March 1956) 43–5, and this second volume will be reviewed in a later number.]

BRONZE AGE CULTURES IN FRANCE: the later phases from the 13th to the 7th century B.C., by N. K. SANDARS. Cambridge University Press, £5 10*s*. [A review of this really splendid book will be published later.]

HOMER AND MYCENAE: property and tenure, by M. I. FINLEY. *Historia*, VI, 1957, 133 ff. [An important study concluding that 'the Homeric world was altogether post-Mycenaean, and the so-called reminiscences and survivals are rare, isolated and garbled. Hence Homer is not only not a reliable guide to the Mycenaean tablets; he is no guide at all.']

BEAKER TYPES AND THEIR DISTRIBUTION IN THE NETHERLANDS, by J. D. VAN DER WAALS. *Palaeohistoria* (Groningen University), IV, 1955, 5–46.

HIGHWAY OF THE SUN, by VICTOR VON HAGEN. Gollancz, London, 1956, 18*s*. [An account of an expedition to trace the Inca roads in Peru. A lively and somewhat journalistic travel book with a sprinkling of archaeology, which needs a pinch of salt here and there. The archaeological results, some of which may prove important, will presumably appear elsewhere.]

CHUGACH PREHISTORY, by FREDERICA DE LAGUNA, Seattle; University of Washington Press, 1956, $6.50. [A report, interesting for Eskimo specialists, on the archaeology of Prince William Sound in Alaska, an area with exceptionally good resources in timber and meat.]

ANCIENT LANDSCAPES: studies in Field Archaeology, by JOHN BRADFORD. G. Bell & Sons, 1957. £4 4*s*.

SICILY, by L. BERNABO BREA. Thames & Hudson, 1957. £1 1*s*.

ZUR ANWENDBARKEIT DER C 14 DATIERUNG in der Vorgeschichtsforschung, by V. MILOJČIĆ. *Germania* 35 (1957), 102–110. [A severe criticism of the method.]

THE TESTIMONY OF THE SPADE, by G. BIBBY. Collins, £1 10*s*. [A first-rate and very readable account of prehistory from the North European point of view by a professional archaeologist. Breaks new ground.]

ADVENTURES IN THE NEAREST EAST, by CYRUS GORDON. Phoenix House, £1 5*s*. [Does much the same on a less comprehensive scale for his chosen field, and is also very readable.]

DIGGING UP JERICHO, by KATHLEEN M. KENYON. Benn, £1 10*s*. [The first account in book form, for a wide public, of these epoch-making excavations, familiar by now to all readers of ANTIQUITY.]

ZU DEN AUSGRABUNGEN IN DEM KELTISCHEN OPPIDUM VON MANCHING, 1955, by WERNER KRÄMER. *Germania* 35 (1957), 32–44.

# Reviews

HAITHABU, EIN HANDELSPLATZ DER WIKINGERZEIT. *By* HERBERT JANKUHN. *Karl Wacholtz, Neumünster*, 1956. *pp.* 256, 46 *illustrations and maps in the text*, 16 *plates with maps and illustrations. DM.* 24.

Hedeby is a well-known Viking age town in the southern part of the peninsula of Jutland. It is situated at the inmost part of Slien fjord, called Hedeby Nor. Here one still can see the impressive semicircular rampart that protected the town. Only its eastern side was open to the water. Serious digging at Hedeby started at the beginning of this century. It culminated with the large-scale investigation carried out in the 'thirties under the leadership of Dr Herbert Jankuhn. As might be expected, these investigations proved to be extremely useful, giving quite a new impression of more peaceful life in Viking age. In 1938 Jankuhn published a book titled *Haithabu, eine germanische Stadt der Frühzeit.* Here was given a summary of the results of the digging in the Viking age town and in the ramparts connected with it. After some later supplementary digging and a new examination of the objects found, a third edition of this book has appeared. That is to say, in reality it is a new book on the same theme. This is indicated in the new title: *Haithabu, ein Handelsplatz der Wikingerzeit.*

(It is to be regretted that German archaeologists have given the name Haithabu to the town in question. Its name in Old Danish should be *Haithabur*, which, put into the oblique case gives *Haithabu*, meaning: *at* the Town on the Heath. Imagine a title like Roma*m*, a residence of the Popes. As the original name has survived on the spot transformed to the modern *Hedeby* I should like to use that in the following review.)

First of all the book deals with the question of the origin and development of a Viking age town like Hedeby. This problem has been discussed eagerly, and in the 'thirties the theories of Walter Vogel were generally accepted. He thought that places like Hedeby, and Birka in Sweden, were planned, founded and fortified all at one time by foreign traders. Birka, he thought, was probably founded by the Frisians, and Hedeby by the Swedes.

Jankuhn has now given quite another answer to these questions, an answer that seems to be the only possible one. Basing his theories on the archaeological finds in north-western Europe he proves his supposition for Hedeby's development, that the important trade route from the Western Europe to the Baltic Sea crossed the peninsula of Jutland as early as in the 7th century just at the place in question. That the semicircular wall protected a habitation of some kind or another has been clear since the first diggings were made. But it was the excavation of the 'thirties that gave the real picture, although it is not yet quite complete. The location of the objects found has proved that the occupation began in the 8th century. It was quite a humble one near the shore covering a space inside as well as outside the northern branch of the semicircular wall. This fact proves that the wall was not planned from the beginning. Later on the settlement grew and was extended in all directions, but mainly westwards, so that in the 9th century it had an almost triangular form. Evidently it followed a road leading to the corresponding harbour to the west. The cemeteries are found west of the houses, but still inside the wall.

It would of course be a great advantage to control a town like this. So it is no wonder that the written traditions tell about repeated conflicts connected with Hedeby. Evidently Danish, German and Swedish rules have alternated in the 10th century. Most certainly one of these conflicts led to the fortification of the town, and some time in this century the semicircular wall was constructed, covering a space of 24 hectares. It included the

old cemeteries but cut off the northern part of the old settlement. Later on, the occupation inside the wall grew so dense that houses were constructed even on the top of the graves, so that by the end of the 10th century we get the impression that the whole 24 hectares were occupied. But this growth cannot be followed in detail. The conflicts must have led to the construction of most of the walls called Danevirke. Yet the southern one was constructed shortly after 800, and the most recent part (the brick wall) in the 13th century, but the rest could reasonably be of the 10th century.

The development of the town shows that Jankuhn is right when he says that the origin of the town depended entirely on the increasing use of the trade-route, and that a convenient meeting place for the merchants formed the first beginning of the town. Merchants coming from the west could drag their ships up the little Trene river to Hollingsted. Here a little harbour has been found together with the remains of a habitation corresponding in time to Hedeby. At Hollingsted the goods had to be unloaded and then transported overland to Hedeby, a distance of only 16 km., where the market was held.

Here the merchants met, one might say from all parts of the world. Even Arabian traders were present in the 10th century, most certainly for buying slaves. This article of commerce seems to have been rather common, as it is mentioned in the *Vita Angarii*. Other kinds of goods were also brought for sale by the Scandinavian merchants: walrus teeth, dried fish, and, of course, furs.

One of the most interesting records about that trade is to be found in Alfred's *Orosius* that includes a report given to Alfred the Great by a Norwegian named Ottar. He is a farmer and a merchant too. In the season he sails from northern Norway (Halogaland) to Hedeby with his merchandize. He is a fine example of how everybody from far and near came to Hedeby.

But, as the finds show, there must have been a considerable number of permanent inhabitants. The cemeteries are calculated to contain about 3500 graves, if not more. But it is not always possible to discover of what nationality the people were, because most of the graves do not contain any gravegoods. A small number, though, are distinguishable as Scandinavians because of the gravegifts, ' tortoise brooches ' and other ornaments, which were in use in Scandinavia only. Some of these Scandinavians buried here must have been artisans living in Hedeby. In a certain quarter of the town there have been found lots of moulds for different Viking ornaments characteristic of Scandinavian countries only. One can easily understand that Hedeby would be the best place for a craftsman to sell his goods. But besides these Scandinavians, there must have been other nationalities, all interested in trade.

The harbour has been successfully found. It seems as though it was protected by a sort of a pier. A double palisade, almost following the 2 m. contour in the water, has been observed. Even a sunken boat of the Viking age has been located, but a minute investigation of these things is still to be done. One must hope it will take place in the not too distant future. A picture of a Viking period town with the habour missing could never be complete.

As far as the finds show, Hedeby flourished in the 10th century. No trace of ' artisanrie ' later than A.D. 1000 has been found. The latest objects found can be dated to the middle of the 11th century. That accords with the saga telling of the plundering of Hedeby by Harald Harderade in the year 1050, and Adam of Bremen says that it was burned by the Wends in 1066.

Why the town lost its importance and declined is not yet clear, but certainly it had something to do with the Slav activity that began about this time.

The description given by Jankuhn of the origins and the flowering of this Viking age

town and of its dependence of the trade routes is of a very high value for the understanding of similar communities, and the author has attained the results of his studies by a conscious, sober and convincing combination of the historical and archaeological material.

In the preface it is said that this is a popular book. So it is, but only in the very best sense of this word, since it is just as useful for the professional as for the layman.

THORKILD RAMSKOU.
National Museum,
Copenhagen

THE PREHISTORY OF EAST AFRICA. *By* SONIA COLE. *Penguin Books Ltd., Harmondsworth, Middlesex*, 1954. *pp.* 294. 43 *text figures*, 10 *maps*, 6 *tables and* 16 *plates. Price* 2*s.* 6*d.*

This book is a most valuable, easily readable and up-to-date account of the prehistoric cultures in East Africa, and is an example of the high standard of accuracy and literary style that we have come to associate with books from the pen of Mrs Cole. The author has managed to include a very considerable quantity of facts and details about the prehistoric cultures of this part of the continent, and at the same time has succeeded in making the book easy to read and of value both to those who seek an introduction to her subject and also to the research worker. In this book are brought together facts scattered throughout a number of scientific books and papers often difficult to obtain; and these facts are presented knit together into a composite whole giving a balanced account of the cultural and climatic succession throughout the whole of Eastern Africa. Although from the title it might be expected that the book would be somewhat confined in its scope the author has included not only Kenya, Tanganyika and Uganda but the Horn and the Sudan and has related her findings to the wider framework of prehistoric research in the continent generally.

The book is divided into ten chapters. After setting the stage with the geographical background and an historical introduction to past research in each of the main areas, Mrs Cole proceeds to describe the climatic setting and the relative chronology of the different prehistoric cultures. It is no easy matter to prevent this from becoming a detailed technical discussion on the merit of the different chronological methods and systems, faunal succession and so on, but this chapter is clear and concise in its presentation. Two very important chapters follow on the early Miocene apes and the South African Man-apes, and their relationships to man's ancestral tree, and on the very rich fossil remains of man from the East African region. Mrs Cole has related these human fossils to the prehistoric cultures as well as to Old World prehistory and the present-day African population generally so that these early human cultures begin to take on again something of the flesh and blood that they once represented. Each of the three main divisions of the African Stone Age is treated in a special chapter. The author's description of the earliest implements is particularly good, though in other places the typological aspects of a culture are perhaps given a little too much prominence though it is difficult to see how these could have been treated otherwise. Following the description of the neolithic cultures the author gives a general and concise account of the rock art of her region with special sections on the dating and significance of this art. The last chapter brings the story up to the end of mediaeval times and introduces the early Iron Age in East Africa. A useful series of tables, distribution maps, and illustrations of cultural material, including two delightful reconstruction drawings by Maurice Wilson, complete the book and make it a very valuable textbook of the prehistory of a special African region and the low price puts it within the means of all.

The book brings out well the great contribution that East Africa has made towards the unravelling of the story of human evolution as well as the amazing wealth of prehistoric material that is to be found there. It shows also how its later cultures are essentially African in origin and development though they probably derived impetus and stimulus from contacts with sea-going peoples who settled along the eastern coastline. Mrs Cole has done African prehistory a great service in compiling this work, which will for long remain an indispensable source book on the East African region, and the publishers are to be congratulated upon their foresight in presenting this book to such a wide public.

J. Desmond Clark.
Rhodes-Livingstone
Museum, N. Rhodesia.

ANCIENT ITALY. *By* G. M. A. Richter. *University of Michigan Press*, 1955. *pp.* 138 *with* 305 *figures.* £6.

This is based on a series of lectures delivered at the University of Michigan in 1952. Introductory chapters discuss the inter-relationship between the Greek, Italic and Etruscan arts in the archaic and classical periods, rightly emphasizing the importance of the Greek element but often simplifying the problems involved in the interest of passing as quickly as possible to the core of the study. This is devoted to an analysis of what is generally called Graeco-Roman art, embracing as it does much that is still popularly thought to be of native Roman rather than Greek inspiration. In every branch of the arts, sculpture, painting, gem-cutting, etc., classical and Hellenistic Greek originals were the models being either copied or adapted to suit Roman taste, and mostly still in the hands of Greek artists in many parts of the Roman empire. Particular attention is paid to the technique of making exact copies of statuary and the pointing process is explained at length. Miss Richter's suggestion that the varied Graeco-Roman versions of earlier statues are due rather to the existence of a similar variety in the Greek originals than to the ingenuity of the copyist is attractive but requires more detailed illustration. The introductory chapters might well have been sacrificed both in this cause, and for a more detailed discussion of the minor arts. The writing is as clear and scholarly as we would expect of the author. Illustration and book production are excellent.

John Boardman.

EXCAVATIONS AT NUZI. Vol. vi: The Administrative Archives. *Edited by* E. R. Lacheman. *Harvard Semitic Series*, xv. *pp.* 16 *and* 177 *plates of cuneiform texts. Harvard University Press* (*Cumberlege*, 1956. £3.)

The nature and importance of the discoveries at the site of Nuzi, near modern Kirkuk, were summarized in this journal by Dr Sidney Smith in 1938 (Antiquity, xii, 425–31). Since that time the publication of the texts has steadily continued. The comparatively small number belonging to the earlier of the two main periods represented at the site were published by T. J. Meek in the third volume of this series. The three following volumes, of which that under review is the most recent, are all devoted to the texts of the later, Hurrian, period, and are all from the pen of Dr Lacheman. The present volume, which is issued in the form of separate plates in a box with sixteen pages of typewritten catalogue, contains administrative archives, mainly lists of persons and inventories of various kinds, but including a few receipts, letters and affidavits. Fifty of them had already been published in *Revue d'Assyriologie*, vol. xxxvi, but are republished because most of that issue was destroyed during the war. The chief value of these documents lies in the evidence

which they provide for the material culture of the people of Nuzi in the 15th century B.C. We learn from them the technical terms for a large number of objects: garments, kinds of cloth and dyes; pieces of armour, trappings for horses, chariots and their parts, and numerous articles of furniture. However, all this information was extracted by Dr Lacheman long before the publication of these copies and is available in the form of a valuable essay contributed by him to the excavation report by R. F. S. Starr (*Nuzi*, vol. 1, 1939). Thus although the publication of the texts as source material was indispensable, the actual amount of new information to be extracted from them is comparatively small; and in any case, as may be seen from Lacheman's essay, many of the special terms are still unintelligible to us. Lacheman is an exponent of the method of copying by drawing over photographic prints which are then dissolved away. In the case of sharply curved tablets, such as no. 12 in this publication, the effect is peculiar; but undoubtedly the method, which has caused some controversy, makes for great accuracy. The ' multilith ' process of reproduction has not improved the appearance of the copies but was apparently the only method by which they could be published at a reasonable price.

O. R. GURNEY.

INDO-EUROPEAN LANGUAGES AND ARCHEOLOGY. *By* H. HENCKEN. (*American Anthropologist, vol.* 57, *no.* 6, *part* 3, *Memoir, no.* 84, *December* 1955.)

Archaeology and comparative philology deal with different aspects of human societies, yet their goal is a common one—the reconstruction of the past. The considerable progress made in recent years in both the branches of research concerned with the Indo-European peoples makes the present survey particularly welcome. It claims to be ' an experiment in treating simultaneously the evidence of language and the evidence of archaeology to see if and how these may be brought to bear on common problems '. The evidence relating to each major language group of the Indo-European family is assembled and discussed systematically, beginning with Celtic and proceeding eastwards. Celtic receives the most detailed treatment, and this is well for it illustrates the dangers and difficulties inherent in the subject, and these are lucidly and sensibly discussed. However, this approach reverses what is likely to have been the chronological order of events, for the extreme west was in all probability reached last by peoples of Indo-European speech. A summary of the findings will thus best begin with the period of ' common Indo-European '. The subject is admittedly highly speculative, and it will be well to state explicitly at the outset the principles of interpretation.

Speech consists in the use of a system of arbitrary sound-signals. By ' arbitrary ' is meant simply that there is nothing in the psycho-physiological constitution of man which makes him utter spontaneously a given string of sounds in response to a given situation. Speech is an intricate mode of social behaviour acquired by slow and laborious training, by the imitation of the older members of the community into which we are born. Since spontaneous independent invention is ruled out, it follows that the resemblant speech habits of two given communities are to be explained only by the hypothesis of an unbroken chain of mimetic processes by which new recruits acquire these peculiar forms of behaviour. This is the simple logic which leads irresistibly from the observation of far-reaching similarities in a group of languages (extending from Celtic in the west to the Tokharian of Chinese Turkhestan) to the hypothesis of a parent Indo-European language from which all the resemblant languages are descended. It should be noted that the hypothesis takes the form of a convergence ' sometime somewhere '. To give precision in time and space requires the co-operation of the archaeologist; but it is possible to provide some further specifications, and recent discoveries have narrowed down the area of possibilities.

Study of common vocabulary long ago led to the conclusion that the Indo-Europeans were familiar with the metal copper, with agricultural operations, with cattle, sheep and their breeding. It is true that some of these words in question might have been diffused after the break-up of the Indo-European ' unity ' (a relative term), but there are indubitable marks of antiquity in certain morphological procedures, and we are fortunate in possessing such criteria for certain new pieces of evidence of vital importance for our assessment of the stage of social organization reached by the Indo-European people before their dispersion. The decipherment of the Linear ' B ' script has shown that Homer is not so much a mirror of heroic society as a screen. Mycenaean society, we can now see, was organized along lines which find their most immediate analogue among the Germanic peoples, though the pattern is also evident in Indo-Iranian and Hittite. But what is vital is that the Mycenaean technical terms for the two main kinds of land-tenure (individual-feudal and collective) are, morphologically speaking, fossils belonging to the most archaic period of Indo-European. There are in the extant remains of Indo-European languages only a few surviving examples of this type of morphology and they all refer to basic sociological concepts (others are ' exchange-gift ', ' wergeld ' and possibly ' funeral feast '). Evidence from the related languages is consistent with the Mycenaean and it is difficult to resist the conclusion that Indo-European society was organized on a complex system of land-tenure, a fact of some importance for the archaeologist since he can rule out the claims of prehistoric cultures which do not offer evidence of stable agricultural settlement.

Hencken examines the archaeological evidence bearing on this hypothesis (p. 49 ff), relying largely on V. Gordon Childe's *Prehistoric Migrations in Europe*, 1950. The settled agricultural economy of the Near East spread to Greece and much of the Balkans in Neolithic times. ' . . . But the temperate zone of deciduous forest was too different an environment to allow this kind of economy to spread far '. West of this area the first convincing candidates for Indo-European status are the Early Bronze Age cultures (roughly 1750–1500) extending from Western Roumania to Southern Germany, in which ' the agricultural economy was by now sufficiently advanced to support miners, smiths, and traders '. . . . ' Finally, if one wishes to look for the further spread of a reasonably settled agricultural society, one has to wait for the spread of the urnfields which began in the last quarter of the 2nd millennium B.C.' It is with this vigorous culture that scholars are inclined to associate the second great period of Indo-European expansion in Europe. ' The vast cemeteries bespeak stable populations . . . there was also a tremendous increase in the use of bronze and a marked development in weapons . . . the horse had been known before, but now its use seems to increase and spread. . . . This was the settled agricultural life that had not until now taken firm and permanent hold in Europe west of Hungary but had been continuous in South-eastern Europe from the Neolithic. . . . The urnfields extend from Roumania to Spain and from Italy to Poland, and their makers were well equipped with bronze weapons. Europe had never before known so powerful a force.' The philologist may add that his evidence suggests that the ancestors of the Celtic, the Germanic, the Italic and the Illyrian peoples once occupied contiguous areas, presumably in Central Europe. He will note, too, that the chronological stages suggested for this westward movement of Indo-European peoples rule out some of the solutions proposed in the section on Celtic. For if the first Indo-Europeans are situated in South-east Europe 1750–1500 B.C., and if Celtic is traced back to the urnfields which began their dramatic expansion after 1250, and if there is general agreement that P–Celtic came into Britain with the Iron Age invasions that started in the 5th century B.C., how can Q-Celtic have been introduced into Ireland 1000 years earlier ?

To compress still further a summary already highly condensed would be to court

unintelligibility. Let it suffice to say that all interested in the prehistory of Indo-European Europe and Asia will be grateful for this painstaking and thought-provoking account of the present stage of research. It clears away the undergrowth and reveals how drastically the area of possibility has been delimited in recent years. Few would now adhere to the extreme Nordic thesis. Asia is no longer the great unknown from which anything convenient to an hypothesis might be plausibly derived. But if south-east Europe now emerges as the likeliest habitat of our linguistic ancestors, one would wish to add to the bibliography the important work which argued this thesis: W. Koppers, *Die Indogermanen- und Germanenfrage*, Salzburg, 1936, a reference which is all the more necessary in that this book was suppressed after the Anschluss.

L. R. Palmer,
Worcester College,
Oxford.

AN OUTLINE HISTORY OF THE NILE VALLEY SUDAN. *By* Chater Bosayley Abd el Galil. *Abu Fadil Press, Cairo*, 1955. *pp.* 308, 7 *Maps.*

This is one of a series of 'Sudan Historical Studies'. The author is the Librarian of the Institute of Sudanese Studies, Cairo University, but there is no indication that the publication is officially sponsored by the University. It is printed in Arabic though there is an English translation of the introductory chapter and a very full and up-to-date list of 'published sources in other than Arabic'. According to the Arabic title-page the book covers the history of the Sudan from the 10th to the 19th century, but in the introduction we read that the study begins with the destruction of the Meroitic kingdom in the 4th century A.D. The first two sections deal with what the author calls the Sennar Sultanate and he follows Crawford (see his *The Fung Kingdom of Sennar*) in tracing the origin of the Fung kings to some region in the south-western part of Eritrea.

The third section of the book is concerned with the Egyptian administration of the Sudan during the 19th century. This part, even more than the others, is, understandably enough, written from the Arab point of view, and it presents a picture surprisingly different from the sombre record with which English readers are familiar. There is, it seems, something to be said even in favour of slavery. To judge from the introduction the author is influenced, as he could hardly fail to be, not only by the prevailing climate of Arab nationalism but also by an 'anti-imperialist' ideology. This, perhaps, is only natural; in the conditions of today dispassionate writing is not to be expected, and, after all, the 'imperialists' have had their innings.

The book, then, is presumably intended to be read chiefly by Arabic-speaking people. Few others, indeed, will be able to read it, for the number of 'western' historians or archaeologists who have any facility in literary—as distinct from colloquial—Arabic is very small. These would have been helped if the price of the book had been stated and some indication given as to where, in England, it could be obtained.

F. Addison.

DAS PFAHLBAUPROBLEM. *Editor:* W. U. Guyan. *Birkhäuser Verlag, Basel*, 1955. *pp.* 334, *Illustrations, maps.* Price: *Swiss francs* 68.65.

The publication reviewed here commemorates the first centenary of pile-dwelling research initiated in 1854 by the Swiss archaeologist Ferdinand Keller. At the same time it attempts to solve the problem of whether the pile-dwellings had been built in water or on dry ground. The publication consists of seven papers dealing with the problem from a variety of aspects covering archaeology and the natural sciences.

The first paper, by J. Troels-Smith, deals with pollen analytical questions relative to the Swiss pile-dwellings. Following a short historical introduction, the methodology of pollen analysis is discussed in some detail. The main discussion evolves around the pollen diagrams obtained from three Neolithic sites: Wauwilermoos, Weiher and Burgmoos. In the final section the conclusions drawn from these diagrams are set forth. The remarkable reduction of beech trees noted in all diagrams seems to have been the result of human activity rather than of climatic changes. The diagrams indicate that the Neolithic inhabitants of these settlements practised agriculture on small permanent plots in artificial forest clearings. The deliberate and extensive cutting down of beech trees also appears to have created conditions favourable for the growth of plants which could profitably be used to feed domestic animals. It seems that the settlements were originally built on somewhat moist ground. On the basis of the pollen diagrams Troels-Smith believes that the settlements were not built in open water.

The second paper, by Max Welten, discusses the pollen diagrams obtained from the Neolithic sites on the Burgäschi Lake in Central Switzerland. The author concludes that the Cortaillod people settled on the Burgäschi Lake about 200 years after the surrounding forests changed from elm-lime-ash to beech trees. There is some evidence in the diagrams for the existence of pre-Cortaillod agriculturalists at the end of the Atlantic period. Archaeologically these have not been traced as yet. The Cortaillod settlements fall into a dry phase between the Atlantic and Sub-Boreal periods. The C–14 dates obtained from a Late Cortaillod settlement on the Burgäschi Lake (2550 and 2150 B.C.) are in agreement with results obtained through pollen analysis. Barring occasional inundations the settlements were certainly located on dry ground. Agriculture was practised on small plots in artificial forest clearings. Field rotation appears to have been common practice.

The third paper, by Werner Lüdi, discusses the flora of the Swiss alpine foothills during the Bronze Age. Comparing the Bronze Age flora with that of today, it appears that with the exception of a few species the two floras were quite similar. The species not occurring in this region today are common in the Mediterranean area. Some of the Bronze Age weeds, too, prefer today somewhat warmer conditions. The pollen diagrams on the whole indicate that climatic conditions during the Bronze Age were not exceptionally dry, but somewhat warmer than today.

The fourth paper, by Hilde Levi and Henrik Tauber, presents four C–14 dates for the Early Cortaillod settlement of Egolzwil near Luzern. The materials used for dating were wood, bark and charcoal, derived from a culture layer embedded between two sterile lake sediments. The mean value obtained from the samples is 4690 $\pm$ 90 B.P. (= 2740 $\pm$ 90 B.C.).

The fifth paper, by Emil Vogt, entitled ' Studies in pile-dwellings ', critically re-examines the old theories of Ferdinand Keller and others as to the original location—in water or on land—of the pile-dwellings. It begins with an excellent historical and methodological review of the problem leading to a restatement of the question in terms of present-day knowledge. This is followed by an exhaustive discussion in 16 sections of the archaeological data and their implications. This discussion evolves around house types, settlement patterns and environmental factors. In the last of these sections the author sums up his findings. It is stated that three kinds of settlements can be distinguished in the Swiss Lake region: swamp settlements, shore settlements and land settlements. Swamp and land settlements are very similar in house construction, and the shore settlements are quite closely related to these two types as well. In all cases the building principles are the same. The buildings were constructed on level ground. The differences in construction may well have arisen from the desire to isolate the construction floors from the moist building

ground. This was done either by raising the floor slightly above ground, or by interpolating a layer of isolating materials between the ground and the construction floor. In either case the mode of construction falls well within the range of house forms built on firm ground. There certainly is no reason to assume construction in water. As to the question of why a large number of the Neolithic and Bronze Age settlements were located in swamps and along lake shores, Vogt believes that these areas were least covered with forest and therefore required a minimum amount of forest clearing for the establishment of settlements. Moreover, the villages were built on the agriculturally least productive ground.

The sixth paper, by Ulrich Guyan, is an admirably complete and systematic site report on the Michelsberg swamp settlement of Thayngen-Weier. A date of 4720 ± 100 B.P. (= 2770 ± 100 B.C.) was obtained for this settlement. The paper is subdivided into five sections covering the excavations and some theoretical discussions on house construction and economy of the site. The author concludes that Thayngen-Weier was not built in open water but on somewhat swampy ground.

The seventh and last paper, by Josef Speck, deals with the excavations carried out at the Late Bronze Age shore settlement of Zug-' Sumpf '. The site yielded two culture layers separated by a sterile lake sediment. Both belong into the Hallstatt B phase, though the lower layer may have had its beginnings during Hallstatt A times. The house types of the upper layer indicate that the settlement was clearly built on dry ground. The foundation floors of the lower settlement suggest a somewhat moister building ground. Certainly neither settlement was built in open water.

*Das Pfahlbauproblem* is profusely illustrated and printed according to the highest standards of publication technique. The papers are clear, systematic and methodologically up to date. The arguments put forth by the authors are on the whole convincing. This publication undoubtedly ranks among the most valuable contributions to the study of Central European prehistory.

Olaf Prufer,
Peabody Museum of Harvard University,
Cambridge, Mass., U.S.A.

GREEK THEATRE PRODUCTION. *By* T. B. L. Webster. *Methuen and Co. pp.* xv + 206 *with* 24 *half-tone plates.* 1956. 25*s. net.*

In this volume Professor Webster attempts to construct from the literary and archaeological evidence a general account of the production of Greek plays from the 6th century B.C. to the Roman period. His study is based upon the whole range of monuments and records connected with the drama, from the remains of ancient theatres to vases, masks and mosaics. The evidence afforded for scenery, staging, or costume is treated chronologically within the geographical areas around the Mediterranean world. More than half the letter-press is allotted to Athens, since she was the acknowledged mistress in these matters. Professor Webster cites the evidence of vases and terra-cotta statuettes from all over the Greek world, from Cyrene and Paestum in the west to South Russia in the East, that present a close similarity in costume in the characters of both tragic and comic plays, to show that Athenian theatre practice was the norm. ' Drama ', he says, ' in this period ' (the 4th century B.C.) ' is an Athenian export and not yet an international possession '. In the several geographical sections the archaeological evidence which they afford is enumerated and the production of particular plays is discussed—the masks worn, the arrangement of doors, the use of stage machinery and similar matters. These detailed studies embody the results of much recent work on the dating and interpretation of the material

remains. Where the evidences considered is so widely extended in time and place generalization is difficult, and it is only in a few pages in the final chapter that Professor Webster attempts to sketch a general picture. This concluding summary is excellent and one could wish that more of the material could have been presented in this way, as there are few scholars who have so deep a knowledge as Professor Webster of both the literary texts and the archaeological material. To the specialist the book will be indispensable.

G. F. Forsey.

ERETZ-ISRAEL: ARCHAEOLOGICAL, HISTORICAL AND GEOGRAPHICAL STUDIES, vol. III, dedicated to the memory of M. D. U. Cassuto, 1883–1951, *Edited by* H. Z. Hirschberg, B. Mazar, H. M. Orlinsky *and* N. H. Tur-Sinai. *pp.* xvi, 266, *with* 10 *plates*. *Central Press, Jerusalem*, 1954. (Received 1955.)

This quarto volume, written in Hebrew, is characteristic of the rich scholarly output in Israel today. The contributors are international. While most of the articles pertain to Palestinology and Old Testament, a number of them embrace other fields such as Assyriology, Egyptology, Greek and especially the history of the Jews in Italy (for the late Professor Cassuto had distinguished himself in this subject as well as in Biblical and Ugaritic studies). Fortunately, many of the fifty-odd articles are also being published in English, in the *Israel Exploration Journal*, so that they will not be limited to readers of modern Hebrew.

Cyrus H. Gordon.
Brandeis University,
Mass.

# Index


Air photographs: Arpi, Apulia, Italy, 153
Fontaine Valmont, Charleroi, Belgium: Romano-Celtic sanctuary, 88
Akkadian tablets in Minoan dress. *By* Cyrus H. Gordon, 237 ff.
ALLCHIN, F. R. The culture sequence of Bactria, 131 ff.
Amphora, Pylos, Greece, *illus.*, 99
Arpi, Apulia, Italy, *illus.*, 167 ff.
ATKINSON, R. J. C. Worms and weathering, *illus.*, 219 ff.
*Australopithecus* as tool-maker, 206 ff.

Bactria: culture sequence, 131 ff.
Battle-axe people, 142 ff.
Beads, 13 ff.
Beads: glass, Pylos, Greece, 99
BEATTIE, *Prof.* A. J. Attack on M. Ventris refuted, 8
Bell beaker folk, 142 ff.
Birsay, Orkney: excavations, 1956...171 ff.
Boitsfort, Belgium: hollow ways, *illus.*, 100 ff.
BRADFORD, JOHN. The ancient city of Arpi in Apulia (note), *illus.*, 167 ff.
BRAIDWOOD, ROBERT J. Jericho and its setting in Near Eastern history, 73 ff.
K. M. Kenyon's *reply to*, 82 ff.
Braves, Beakers and Battle-Axes. By H. W. M. Hodges, *illus.*, 142 ff.
Brinton, David Garrison, 215
Bronze age villages. Salina, Aeolian Islands, *illus.*, 9 ff.
Brummagen Philhellene, The. *By* A. J. B. Wace, 85 ff.
BURN, A. R. Wild doings in the theological college, 93 ff.

Campignian Tradition and European Flint-mining, The. *By* W. Creighton Gabel, 90 ff.
Castillejo, Spain: hill-fort, *map*, 27
Castro de las Espinillas, Spain: hill-fort, *illus.*, *map*, 27 ff.
Castro de Miranda, Spain: hill-fort, *map*, 30
Cavalier, Madeleine. Salina: a prehistoric village in the Aeolian Islands, *illus.*, 9 ff.
CHILDE, V. GORDON. Civilization, cities and towns (note), 36 ff.
The evolution of society, 210 ff.
*Obit.*, 187
Chubb, George Fanshawe, 85 ff.
Civilization, cities and towns, 36 ff.
Civil Service archaeology: a review. *By* Sir Mortimer Wheeler, 234 ff.
COLLINGWOOD, R. G. Digging-sticks in Java (note), 39 ff.
Contents, 1
Copper rolls, Qumran, Palestine, 1 ff.
COTTER, JOHN L. Excavations at Jamestown, Virginia, *illus.*, 19 ff.
'Cromerian industry,' 202
Culture sequence of Bactria, The. *By* F. R. Allchin, 131 ff.

Dagger, Pylos, Greece, *illus.*, 99
DANIEL, GLYN. The 150th anniversary of the Danish National Museum (note), 169 ff.
Danish National Museum: 150th anniversary, 169 ff.
Darwinism, 188 ff.
Diffusion, Evolution and, 214 ff.
Digging-sticks, Java, 39 ff.
Diodorus Siculus quoted, 12

Earthworks: erosion and silting, 227
Earthworms: archaeological significance of, 219 ff.
Editorial notes, 1
Evolution and diffusion. *By* Leslie A. White, 214 ff.
Evolution and man. *By* Ronald Singer, 188 ff.
Evolution of society, The. *By* V. Gordon Childe, 210 ff.
Excavations, unpublished: editorial strictures, 121
Basilica of St Peter, Rome, 15 ff.
Birsay, Orkney, 171 ff.
Glastonbury Abbey, 171
at Jamestown, Virginia. *By* John L. Cotter, *illus.*, 19 ff.
Jarlshof, Shetland, 234 ff.
Pylos, Greece, tholos tomb, *illus.*, 97 ff.

Festschrifts, 2
First Universities, The. *By* Humphrey Humphreys, 33 ff.
Flint-mining, 90 ff.
Flint: natural flaking, 200 ff.
Fontaine Valmont, Charleroi, Belgium: Romano-Celtic Sanctuary, *illus.*, 103 ff.
Fore and aft rigging in the Roman Empire, 39
Four Spanish Hill-forts. *By* A. H. A. Hogg, *illus.*, 25 ff.

Gabel, W. Creighton. The Campignian tradition and European flint-mining, 90 ff.
Glastonbury Abbey: excavations, 1956...171
Goats, Early, *illus.*, 38 ff.
Gordon, Cyrus H. Akkadian tablets in Minoan dress, 237 ff.
Notes on Minoan Linear A., 124 ff.
Graffiti of ships at Tarxien, Malta. By Diana Woolner, *illus.*, 60 ff.

Hawkes, Jacquetta. The Longstone, Mottistone, *illus.*, 147 ff.
Hill-forts, Spain, *illus.*, 25 ff.
Hodges, H. W. M. Braves, beakers and battle-axes, *illus.*, 142 ff.
Hogg, A. H. A. Four Spanish hill-forts, *illus.*, 25 ff.
Hollow ways, Boitsfort, Belgium, *illus.*, 100 ff.
Houses, Skye, *illus.*, 155 ff.
Humphreys, Humphrey. The First Universities, 33 ff.
Hutchinson, R. W. Fore and aft rigging in the Roman Empire (note), 39

Indians, North American, 142 ff.

Jamestown, Virginia: excavations, *illus.*, 19 ff.
Jarlshof, Shetland: excavations, 234 ff.
Java: digging-sticks, 39 ff.
Java man, 195 ff.
Jericho and its setting in Near Eastern history. *By* Robert J. Braidwood, 73 ff.

Kenyon, Kathleen M. Reply to Professor Braidwood, 82 ff.

La Portella, Salina, Aeolian Islands: Bronze Age village, 9 ff.
Lake-dwellings: theories disputed, 68 ff.
Las Cogotas, Spain: hill-fort, *illus.*, *map*, 28 ff.
Lee, N. E. The Sutton Hoo ship built in Sweden ? (note), 40 ff.
Linear A script, 123, 124 ff., 237 ff.
Lipari, Aeolian Islands, 9 ff.
Longstone, Mottistone, The. *By* Jacquetta Hawkes, *illus.*, 147 ff.

Malan, B. D. Old and new rock engravings in Natal, South Africa: a Zulu game, *illus.*, 153 ff.
Maloisel, Lancelot de. Lost book on Canary Islands, 3
Marinatos, S. Excavations near Pylos, 1956, *illus.*, 97 ff.
Megaliths: Mottistone, I. O. W., *illus.*, 147 ff.
Ministry of Works: conservation of antiquities, 234 ff.
Moir, Reid, 201
Morgan, Lewis H., 214
Mottistone, I.O.W.: Longstone monument, *illus.*, 147 ff.
Mycenaean potsherds, La Portella, Salina, 12
Mycenaean records: a review. *By* T. B. L. Webster, 4 ff.
Notes on Minoan Linear A. *By* Cyrus H. Gordon, 124 ff.

Oakley, Kenneth. Tools makyth man, *illus.*, 199 ff.
Old and new rock engravings in Natal, South Africa: a Zulu game. *By* B. D. Malan, *illus.*, 153 ff.
Oldowan industry, 204
Origin of Species, 185

Peking man, 195 ff.
Perthes, Boucher de, 185
Petrie, Sir Flinders, quoted, 122
Philon of Byblos quoted, 93 ff.
Pile-dwellings, Swiss, 68 ff.
*Pithecanthropus erectus*, 194 ff.
Pithos, six-handled, La Portella, Salina, *illus.*, 11
Plains Indian society, 142 ff.
Ploughshares and coulters, Roman and Early Iron Age, sought, 3
Pottery: Bronze Age, Italy, *illus.*, 11
Cretan, 100
Mycenaean, *illus.*, 12
Panarean, *illus.*, 10
Pylos, Greece: tholos, 58
tholos tomb excavated, *illus.*, 97 ff.

Qumran caves, Palestine: copper rolls, 1 ff.

RADFORD, C. A. RALEGH. Excavations at Birsay, 1956 (note), 171 ff.
Excavations at Glastonbury Abbey, 1956 (note), 171
Radiocarbon dating, Jericho, 74 ff.
REED, CHARLES A. Comment on ' early goats,' *illus.*, 38 ff.
Rock engravings: modern Zulu, *illus.*, 153 ff.
Romano-Celtic sanctuary, Fontaine Valmont, Charleroi, Belgium, *air photo.*, 88, 103 ff.
Rome: excavations at St Peter's, 15 ff.
Roof trusses, Skye, *illus.*, 161

Salina: a prehistoric village in the Aeolian Islands, *By* Madeleine Cavalier, *illus.*, 9 ff.
SALWAY, P. A Romano-Celtic sanctuary in Belgium (note), *illus.*, 103 ff.
Scarfed trusses, Syke houses, *illus.*, 160 ff.
SCOLLAR, IRWIN. Hollow ways at Boitsfort, near Brussels (note), *illus.*, 100 ff.
*Scottish Studies:* first number, 59
Seals, Pylos, Greece, *illus.*, 88
Shipping, early, 57
Ships: fore and aft rig, Roman, 39
graffiti at Tarxien, Malta, *illus.*, 60 ff.
Shrine of St Peter, The. *By* Erik Sjöqvist, 15 ff.
SINGER, RONALD. Evolution and man, 188 ff.
SJÖQVIST, ERIK. The Shrine of St Peter: a review, 15 ff.
Skulls of primates, *illus.*, 194, 195
Skye House, The. *By* James Walton, *illus.*, 155 ff.
' Split peas,' 225
Sterkfontein, Transvaal: stone artifacts, *illus.*, 206
STONE, *Dr* J. F. S. On beads from La Portella, 13
Stratigraphy: falsified by earthworm activity, 219 ff.
Sutton Hoo ship built in Sweden ? 40 ff.
Swiss Pile-dwellings. *By* E. Vogt, 68 ff.

Tarxien, Malta: graffiti of ships, *illus.*, 60 ff.
Tell es-Sultan, Palestine, 73 ff.
Tools: effect upon human evolution, 185 ff.
Makyth Man. *By* Kenneth Oakley, *illus.*, 199 ff.
pebble, 205
stone and steel, *illus.*, 144
Treasure, buried, 1 ff.
Turf lines, 225

Universities, The First, 33 ff.

VENTRIS, Michael, 4 ff.
VOGT, E. Swiss pile-dwellings, 68 ff.

WACE, A. J. B. The Brummagen philhellene, 85 ff.
WALTON, JAMES. The Skye house, *illus.*, 155 ff.
Weathering of subsoils, 228
WEBSTER, T. B. L. Mycenaean records: a review, 4 ff.
WHEELER, Sir MORTIMER. Civil service archaeology: a review, 234 ff.
WHITE, LESLIE A. Evolution and diffusion, 214 ff.
Wild doings in the theological college. *By* A. R. Burn, 93 ff.
WOOLNER, DIANA. Graffiti of ships at Tarxien, Malta, *illus.*, 60 ff.
Worms and weathering. *By* R. J. C. Atkinson, *illus.*, 219 ff.

# Reviews of Books

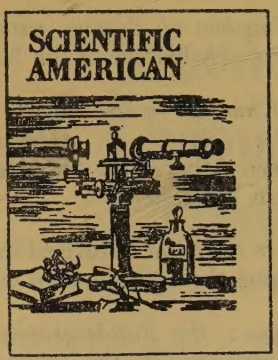

PAGE

*Acta archaeologica*, vol. xxv. 1954. Copenhagen: Ejnar Munksgaard. 38 *crowns* – 177
Alimen (H.). *Préhistoire de l'Afrique*. 1955. Paris; Boubée. 3000 *fr.* – – – 49
Barb (A. A.). *Mens sacra*. 1956 – – – – – – – – – – 116
Beazley (J. D.). *Attic black-figure vase-painters*. 1956. Oxford Univ. Press. £6 6*s*. – 179
Bennett (E. L.). Ed. *The Pylos tablets*. 1955. Princeton Univ. Press (Cumberlege). £2 112
Blair (Peter Hunter). *An Introduction to Anglo-Saxon England*. 1956. Cambridge Univ. Press. 30*s*. – – – – – – – – – – – – – 105
Bogaers (J. E. A. Th.). *De Gallo-Romeinse tempels te elst in de Over-Betuwe*. 1955. 'S-Gravenhage – – – – – – – – – – – – 181
Bosayley (Chater). *An Outline history of the Nile Valley Sudan*. 1955. Cairo: Abu Fadil Pr. – – – – – – – – – – – – 248
Breuil (H.). *The Rock paintings of Southern Africa*. Vol. 1. 1955. Faber and Faber. £5 5*s*. – – – – – – – – – – – – – 51
Burkitt (M. C.). *The Old Stone Age*. 1955. 3rd Ed. Bowes and Bowes. 21*s*. – – 178
Childe (V. Gordon). *Piecing together the past*. 1956. Routledge and K. Paul. 18*s*. – 52
Clarke (W. E. Le Gros). *The Fossil evidence for human evolution*. Chicago University – 43
Cole (Sonia). *The Prehistory of East Africa*. 1954. Penguin Books. 2*s*. 6*d*. – – 244
Driver (G. R.) *and* Miles (John C.), Eds. *The Babylonian laws*. Vol. 2. Oxford: Clarendon Press. 63*s*. – – – – – – – – – – – 183
Dunham (Dows). *The Royal cemeteries of Kush*. Vol. 2: *Nuri*. Boston, Mass.: Museum of Fine Arts. $32.50 – – – – – – – – – – – 114
Finley (M. I.). *The world of Odysseus*. Chatto and Windus. 15*s*. – – – – 42
Gabriel (Mabel M.). *Livia's garden room at Prima Porta*. New York Univ. Press. $12 54
Guyan (W. U.). Ed. *Das Pfahlbauproblem*. Basle: Birkhäuser Verlas. *Swiss fr.* 68.65 248
Hencken (H.). *Indo-European languages and archaeology*. 1955. American Anthropologist 246
Hirschberg (H. Z.) *and other* Eds. *Eretz-Israel*. Vol. III. 1954. Jerusalem: Central Press – – – – – – – – – – – – – – 251
*History and archaeology in Africa: a report*. 1955. University of London – – – 47
Hondius-Crone (Ada). *The Temple of Nehalennia at Domburg*. 1955. Amsterdam: Meulenhoff – – – – – – – – – – – – – 180
*Inventria archaeologica: Ages des metaux, France*. Anvers, De Sikkel. *B. fr.* 75 – – 110
*Inventaria archaeologica: Great Britain, 1st set*. 1955. Garraway. 10*s*. – – – 109
*Jahrbuch des Römisch-Germanischen*. 1954. *Zeutralmuseums, Mainz*. *DM*. 30 – – 110
Jankuhn (Herbert). *Haithabu: ein Handelsplatz der Wikingerzeit*. 1956. Neumunster: Wacholtz. *DM*. 24 – – – – – – – – – – – 242
Lacheman (E. R.) Ed. *Excavations at Nuzi*, vol VI. 1956. Harvard Univ. Press (Cumberlege). £3 – – – – – – – – – – – – – 245
Lal (B. B.). *Excavations at Hastinapura and other explorations in the Upper Ganga and Sutlej basins*, 1950–52. Archaeological Survey of India, 1954–55. *Rs*.9 or 10*s*. – 108
Lee (Norman E.). *Travel and transport through the ages*. Cambridge Univ. Press. 12*s*. 6*d*. 48
Lehmann (Karl). *Samothrace: a guide to the excavations and the Museum*. 1955. New York Univ. Press. $2.50 – – – – – – – – – – – 48
*Mission de Ras Shamra*. Tome VI. 1955. Paris: Klincksieck. *Dutch guild* 70.40 – 115
Motes (J. M. de). *El Yacimiento Hallstattico de Cortes de Navarra*. 1954. Pamplona. 150 *pesetas* – – – – – – – – – – – – – 174
Neugebauer (O.). *Astronomical cuneiform texts*. 1955. 3 vols. Lund Humphries. £5 5*s*. 106
O'Riórdáin (Seán). *Lough Gur excavations*. *Proc. Roy. Irish Academy*. 25*s*. – – 111
Pettazzoni (Rafaelle). *The All-knowing god*. 1956. Methuen. £3 – – – – 183

PAGE

Pritchard (James B.) Ed. *Ancient near eastern texts relating to the Old Testament*. 2nd ed. 1955. Princeton Univ. Press (O.U.P.). £7 – – – – – – – 120
Rajkay (L. I.). *La Puerta de los Leones de Micenas y la Columna Minoana*. 1955. Caracas 53
Reimschneider (M.). *Die Welt der Hehiter*. Stuttgart: Gustav Kilpper Verlag – – 43
Reisner (G. A.). *A History of the Giza necopolis*. Vol. 2: (Hetep-heres). Harvard Univ. Press (Cumberlege). £10 – – – – – – – – – – 53
Richardson (L.). *Pompeii: the Casa dei Dioscuir and its painters*. 1955. Rome: American Academy – – – – – – – – – – – – – – 182
Richter (G. M. A.). *Ancient Italy*. 1955. Univ. of Michigan Press. £6 – – – 245
Rumpf (A.). *Archaologie II: Die Archaologen-sprache, die Ahtiken Reproduktionen*. 1956. Berlin: Sammlung Göschen Band 539. 4*s*. 6*d*. – – – – – – – 112
Sarkar (S. S.). *The Aboriginal races of India*. 1954. Calcutta: Bookland Ltd. *Rs*. 12 – 178
Savory (H. N.). *The Excavation of a neolothic dwelling*. Cardiff: National Museum of Wales – – – – – – – – – – – – – – 109
Schmökel (H.). *Ur, Assur and Babylon*. 1955. Stuttgart: Gustav Kilpper Verlag – 43
Shinnie (P. L.). *Excavations at Soba*. 1955. Khartoum: Sudan Antiquities Service. 10*s*. 107
Struve (Karl W.). *Die Einzelgrabkultur in Schleswig-Holstein und ihre Kontinentalen Beziehungen*. 1955. Neumunster: Wachholtz. *DM*.30 – – – – – 176
Tackenburg (Kurt). *Fundkarten zur Vorgeschichte der Rheinprovinz*. Bonn: Rheinisches Landesmuseum. *DM*.9.50 – – – – – – – – – – 111
Thomson (George). *The first philosophers*. 1955. Lawrence and Wishart. 27*s*. 6*d*. – 44
Vaufrey (R.). *Prehistoire de l'Afrique*, Tome I. 1955. Paris: Masson et Cie – – 117
Webster (J. B. L.). *Art and literature in fourth-century Athens*. 1956. Univ. of London. 25*s*. – – – – – – – – – – – – – – 113
—— *Greek theatre production*. 1956. Methuen. 25*s*. – – – – – – 250
Willcox (A. R.). *Rock paintings of the Drakensberg*. Max Parrish. 80*s*. – – – 56
Willey (Gordon R.) *and* McGimsey (C. R.). *The Monagrillo culture of Panama*. Harvard University. $4.65 – – – – – – – – – – – – 113
Winick (Charles). *Dictionary of anthropology*. 1957. Peter Owen. £2 10*s*. – – 173

*To keep informed*
*on work*
*in fields outside*
*your own*




. . . read SCIENTIFIC AMERICAN. Each month this magazine presents original, first-hand accounts written by scientists on work going forward in all departments of science. The articles are presented with the collaboration of the editors, in non-technical language, and are handsomely and informatively illustrated.

In the U.S.A. during the past eight years, SCIENTIFIC AMERICAN has gathered a unique audience of more than 135,000 regular readers. They include scientists and engineers in every field, who depend upon its pages to keep informed on fields outside their own.

The New York *Times* has called SCIENTIFIC AMERICAN "one of the most remarkable ventures in the history of scientific publication."

Readers of ANTIQUITY are invited to subscribe; send your order and payment to the address below.

*Subscription rates* | one year $8 two years $14 three years $18

2 West 45th Street, New York 36, N.Y.

**SCIENTIFIC AMERICAN**

The following books by the Editor of ANTIQUITY are available and may be obtained from H. W. Edwards, Booksellers, Ashmore Green, Newbury, Berks, England.

1. *Topography of Roman Scotland North of the Antonine Wall*, 1949. £2. [A full account, with many detailed plans of the Roman roads and forts.]

2. *The Fung Kingdom of Sennar, with a Geographical Account of the Middle Nile Region*, 1951. £2 17*s*. [A history of the Northern Sudan between 1504 and 1820.]

3. *Castles and Churches in the Middle Nile Region.* Sudan Antiquities Service, Occasional Papers No. 2. 1953. 5*s*. [Describes the Christian monuments of this little-known region between Atbara and Abu Hamed, which the author surveyed in the field in 1952 for the British Academy.]

4. *Archaeology in the Field.* Revised impression, 1956. £2 5*s*. [A handbook of field archaeology based upon the author's own work over a period of fifty years.]

5. *Said and Done: the Autobiography of an Archaeologist*, 1955. £1 1*s*. [Describes the various experiences of the author in peace and war, including the foundation of ANTIQUITY.

6. *The Eye Goddess*, 1957. £2 10*s*. [Traces the origin and spread of a prehistoric agricultural fertility cult from the Middle East to Spain, Ireland, Britain and North-West Europe.]

Also available: General Index to ANTIQUITY, vols. I–XXV, 1927–1951. 1956. £2 10*s*.

H. W. EDWARDS (Booksellers) Ltd.

Wish to Buy Libraries or Single Volumes of

BOOKS PRINTED BEFORE 1850

*Careful Attention Given to Lists*

*Liberal Commission paid for suitable introductions to Libraries*

*Please write to*

H. W. EDWARDS (Booksellers) Ltd.

ASHMORE GREEN, NEWBURY, BERKS.

# BRITISH MUSEUM

Recent publications include the following :

**CHRONICLES OF THE CHALDAEAN KINGS (626–556 B.C.) IN THE BRITISH MUSEUM.**
By D. J. Wiseman—1956.
*Pp. xi, 99, and 21 plates* £1 5s.

**CATALOGUE OF THE MOHAMMADAN COINS IN THE BRITISH MUSEUM. Vol. II : Catalogue of the Arab-Byzantine and Post-Reform—Umaiyad Coins**
By J. Walker.
*Pp. civ, 329, and 31 plates* £5 10s.

**CUNEIFORM TEXTS FROM CAPPADOCIAN TABLETS : Part V**
Edited by S. Smith and D. J. Wiseman.
*Pp. 4, and 50 plates* £1 7s. 6d.

**CATALOGUE OF NIMRUD IVORIES IN THE BRITISH MUSEUM**
By R. D. Barnett—1957.
*Pp. 252, and 132 plates* £8 8s.

**UR EXCAVATIONS : Vol. IV : THE EARLY PERIODS**
By Sir Leonard Woolley—1956.
*Pp. 225, and 83 plates* £5 15s.

Handbooks now available include :

**Roman Coins, 5s. Antiquities of Roman Britain, 5s. Flint Implements, 4s. 6d. Inventaria Archaeologica : Bronze Age Hoards in the British Museum, 10s. Late Prehistoric Antiquities of the British Isles, 6s. Sutton Hoo Ship Burial, 7s.**

Obtainable from most booksellers or from

**The DIRECTOR, British Museum, LONDON, W.C.1**

Lists of publications on request



# Man's Journey Through Time [30s.]

L. S. PALMER

In this book Professor Palmer shows how man's physical evolution and cultural development may be expressed quantitatively and graphed against the accepted time-scale of the last million or so years. An extrapolation of the observed trends into the future is then suggested.
*Fully illustrated*

# The Prehistory of Africa [63s.]

H. ALIMEN

The primary importance of Africa in the history of man is clearly shown in this book. Professor Alimen outlines the results of the latest research, and covers the ancient geography of Africa, its geology, its climate in former times its fossils and stone implements, and its primitive cultures.
*Fully illustrated*

**Hutchinson** SCIENTIFIC AND TECHNICAL PUBLICATIONS

THE NEW BOOK BY THE EDITOR OF 'ANTIQUITY'

# THE EYE GODDESS

**by O. G. S. CRAWFORD,** C.B.E., LITT.D., F.B.A.

What religion did they have in Britain and the West in prehistoric times? What is the significance of the symbols carved on the stones of the megaliths? Are some of them related, however distantly, to the Eye Goddess of Syria? In this fascinatingly speculative book with its gallery of curious illustrations, Dr O. G. S. Crawford tries to answer such questions as these. Starting in the Most Ancient East with Ishtar and the Eye Goddess of Brak, the author traces a possible spread of the Eye Goddess westwards to Troy, Greece and thence to Southern Italy, Sicily and Spain. He discusses a further possible expansion of the cult to North Africa and the Canary Islands in one direction, and to Brittany, Britain, Ireland and N.W. Europe in others.

It is a long time since a book of this kind was written, and archaeology has revealed much in the interval. In order to check the facts the author has visited many of the countries and islands concerned, photographing and making observations. Over fifty of the photographs are reproduced in this book, all relating to the Eye Goddess, together with specially prepared maps and drawings. About half of them are published here for the first time, and of the rest many are available only in rare foreign publications.

Dr. Crawford says: "I have written this book for the readers of *Antiquity*."

CONTENTS OF 'THE EYE GODDESS'

HUNTING, PLOUGHING & PRAYING
THE FERTILITY CULT IN SYRIA
WESTWARDS TO THE AEGEAN, GREECE & ITALY
ON TO IBERIA
IBERIA TO BRITTANY
GAVR INIS
MORE BRETON SYMBOLS; ALSO BAETYLS & QUERNS
IRELAND
FROM IRELAND TO BRITAIN
AFRICA
THE CANARY ISLANDS
SOUTHERN ETHIOPIA
MODERN SURVIVALS

*10″ × 7½″, 208 pp. with 48 pp. plates, frontispiece and 46 line drawings. 50s. net.*

Through all bookshops or from H. W. Edwards, Ashmore Green, Newbury, Berks.

*Published by* PHOENIX HOUSE LTD., 38 William IV Street, London, W.C.2

# ROMAN SILCHESTER

**GEORGE C. BOON**

This is the first book to give the full account of all that is now known about the Roman-British town of Calleva, from the 19th-century excavations up to date. An introduction by Mrs M. Aylwin Cotton discusses the Belgic origins of the town, and the text describes both the site itself and the layout, buildings and life of the town under the Roman occupation. Bibliography, very full annotation and a folding plan of the site. Demy 8vo. 245 pp. 60 halftone & line illus. 25s. net.

55 QUEEN ANNE STREET · LONDON W.1 PARRISH

Printed in Great Britain for ANTIQUITY PUBLICATIONS LIMITED by The Whitefriars Press Ltd., London and Tonbridge